NEW-YORK

MEDICAL AND PHYSICAL JOURNAL.

VOL. I.] OCTOBER, NOVEMBER, AND DECEMBER, 1822. [No. IV

CASE I.

ART. I. CASE *of* OSTEO SARCOMA—*in which the right side of the lower jaw was removed successfully after tying the* CAROTID ARTERY. By VALENTINE MOTT, M.D. Professor of Surgery in the University of New-York.

CATHARINE BUCKLEW, the subject of the following operation, was an interesting young woman, aged about seventeen years, of a healthy appearance and good constitution.

She says that about two years since, a swelling commenced behind the last molar tooth of the lower jaw, attended with acute pain about the angle of the jaw, that continued about three weeks; at which time it left her without any evident resolution of the inflammation. At this period there was no inflammation of the integuments, nor could any pus be discovered either on the cheek or about the bone within the mouth. Some domestic applications were made to the cheek, but the tumefaction continued to increase, and assumed a smooth, hard, and bony character.

About twelve months after its commencement she applied to a physician in New-Jersey, who advised her to apply blisters to

the cheek, and the use of topical applications of caustic to the tumour, together with a general antiphlogistic constitutional treatment. After having submitted to this course for two months without experiencing any benefit, she came to this city, and became my patient.

The first molar tooth came away early in the disease, and the second soon followed; then, three or four of the other teeth of that side of the lower jaw. She states, that previously to this disease she had never had a decayed tooth.

No fluctuation was to be felt at any time in the tumour. She had no constitutional symptoms as the effect of this disease, nor any inordinate headache on that side. The lymphatic glands of the neck were however swollen, during the continuance of the inflammation in the early part of the disease; but they disappeared as soon as the pain subsided.

When she came under my care, the tumour extended from the root of the coronoid process to the second bicuspid tooth, elevated nearly an inch above the level of the teeth, and spreading considerably wider than the alveolar process. Its appearance was smooth, and to the touch somewhat elastic, though firm. An incision on each side of the alveolar margin, with a scalpel, enabled me pretty readily to remove the tumour with a gum-lancet to the level of the jaw-bone. The tumour, on examination, contained many cartilaginous and osseous spiculæ, and in the substance of it was imbedded one of the molar teeth in a perfectly sound state.

About three weeks after this operation a small portion, of the size of a nutmeg, which had granulated and grown rapidly, was taken off, and soon after she retired to the country, and remained in a very comfortable state for several months. The tumour began now to re-appear, and continued to increase gradually in every direction.

The tumour at present, (Nov. 10th, 1821,) has the same firm and slightly elastic feel which characterized it in the early stage, involving all the right side of the inferior maxillary bone. Projecting outwards, it produces great convexity of the cheek: upwards it divides into two portions, the outer and

longest reaches up to the os malæ, and between the two is a considerable furrow, formed by the teeth of the upper jaw, which occasions an abrasion and constant discharge; the latter though offensive, does not appear to be acrid or irritating: downwards it comes nearly in contact with the thyroid cartilage; inwards it extends beyond the middle line of the mouth, pushing the tongue and uvula very much to the left side, having the velum pendulum palati of the right side attached to it in its whole course. The inward portion is considerably raised above the level of the tongue when the mouth is opened.

The posterior extremity of the tumour has encroached so much upon the passage leading into the posterior fauces, and the pressure of the lower parts upon the larynx is so considerable, as to render deglutition very difficult; and from the great difficulty of mastication, she has been compelled for some time to subsist upon liquid aliment. Her speech is considerably interfered with in consequence of the displacement of the tongue. She experiences no pain in any part of the tumour.

The gradual increase of the disease rendering mastication and deglutition more difficult and distressing, she is very desirous of knowing if an operation could not be performed which might extend to her some chance of life; observing, that with the constant growth of the tumour, such as has taken place for a few weeks past, she should not be able to swallow any thing in a short time. Fully aware of the dangerous nature of the novel operation her case requires, she is determined to submit to it, and hazard the consequences; the uncertain result of which I carefully explained to her, and informed her, that she might die during the performance of the operation; but that I believed it to be both practicable and proper.

After preparing the system for about a week with light diet, and the exhibition of several doses of neutral salts, to obviate any great degree of inflammation, the operation was commenced about 11 o'clock on the morning of the 17th.

As most of the important branches of the external carotid artery would be interfered with in the course of this operation, I believed it most prudent to pass a ligature around the trunk

of the carotids as a first and preparatory step. This would not only enable me to go through it with more safety to the patient, but appeared the most important of all means to avoid inflammation. Indeed inflammation was much to be dreaded, from the immense extent of the external incision, and the violence which would necessarily be done to the tongue, palate, and pharynx.

From these considerations, I felt it doubly important to intercept the current of blood through the common carotid, and from what I had observed to attend the application of ligatures to the large arteries of the extremities, in cases of severe injuries, by preventing inflammation, I thought great advantage would attend it in this case, as I am satisfied will be fully shown.

An incision about two inches and a half long was made a little below the thyroid cartilage on the inner edge of the sterno-cleido-mastoideus muscle, and after exposing the carotid, a single ligature was passed under it and tied. In this situation it was deemed most proper to tie the carotid in order to prevent the second part of the operation from interfering with the first incision. Very little blood was lost, only one small cutaneous branch at the lower angle of the wound required a ligature; yet she became pale and almost pulseless during, and immediately after, the operation, notwithstanding her position was recumbent. She submitted to the operation with great firmness and resolution, but her mind soon became agitated and perturbed to a great degree, and it seemed altogether impossible for her to regain her former fortitude. The operation was suspended, and some cordial was administered, but it failed to remove from her mind the presentiment that any further proceeding at present would be fatal. In this state of remarkable agitation I resolved not to proceed, and informed her that with such fears as she then entertained, the result was to be dreaded. The wound was then dressed, and she was put in bed, faint and exhausted.

After recovering a little, I apprised her that this was only preparatory to the most important part of the operation, and

that what had been done would prove of little or no benefit to the disease, and urged her seriously to consider of it, and if possible make up her mind to submit to the performance of the remaining part, which should by no means be deferred longer than the following day.

One o'clock, P. M.—She is still pale, and in a cold sweat; pulse has not recovered itself; and when asked, nodded that she felt some uneasiness.

Seven o'clock, P. M.—Much more collected; pulse natural; no uneasiness whatever, except some obtuse pain about the wound in breathing, and in swallowing saliva; no increase of heat; left a student to watch with her through the night, and again took leave, earnestly recommending to her private consideration the expediency of submitting to the remainder of the operation.

18th.—Seven o'clock, A. M.—Found her this morning in a very composed state of mind; having slept well, and free from fever. Upon putting the question, would she submit to the remainder of the operation? she nodded assent with much apparent decision, and said she was determined to undergo it.

At ten o'clock, finding my patient cheerful and resolute, she was again placed upon the table, and, in the presence of William Anderson, surgeon, Dr. Hosack, and a number of other gentlemen, the operation was continued. Feeling for the condyloid process, an incision was commenced upon it, opposite the lobe of the ear, carried downwards over the angle of the jaw in a semicircular direction along the lower part of the tumour, as it rested upon the thyroid cartilage, and terminated it about half an inch beyond the angle of the mouth, on the chin. The termination of this incision upon the chin, was just above the attachment of the under lip to the bone, and the mouth was thereby laid open. I now extracted the second incisor tooth of that side, as it was in a sound part of the bone, and, after separating the soft parts from the side of the chin, and laying bare the bone, I introduced a narrow saw,

about three inches long, similar to a key-hole saw, from within the mouth, through the wound, and sawed through the jaw-bone from above downwards. The lower part of the tumour was then laid bare, by cutting through the mylo-hyoid muscle, and the flap of the cheek carefully separated and turned up over the eye. This exposed fully to view the whole extent of the tumour as it rose upwards to the os malæ. After the integuments were carefully dissected from the parotid gland, the masseter muscle was detached from its insertion, until it came to the edge of this gland, then separating a thin plane of the fibres of this muscle, I now readily raised the parotid, without wounding it at this part. The maxilla inferior was now laid bare just below its division into two processes, and it appeared sound. To facilitate the sawing of the bones, it was necessary to make a second incision, about an inch long, close to the lobe of the ear, and terminating at the edge of the mastoid muscle; then, with a fine saw, made for the purpose, smaller and more convex than Hey's, I began to saw through the bone, obliquely downwards and backwards, and finished with one less convex. The latter part of the sawing was done with great caution, to avoid excruciating pain from the laceration of the inferior maxillary nerve. When the bone was sawed through, the two processes were observed to be split asunder, and the coronoid to be drawn up by the action of the temporal muscle.

An elevator was now introduced where the bone was divided at the chin, by which the diseased portion was raised, when, with a scalpel passed into the mouth, the tumour was separated from the side of the tongue, as far back as the posterior fauces, from the velum pendulum palati and pterygoid processes. This loosened it very much, so that it could be turned upon the side of the neck. It was then separated from the parts below the base of the jaw, and also from the pharynx, and detached at the posterior angle, carefully avoiding the trunk of the internal carotid and deep-seated jugular vein, both of which were exposed.

The diseased mass, being now separated above and below, was turned up, the pterygoid muscles detached, and the third branch of the fifth pair of nerves divided from below, a little above the foramen at which it enters the bone. By this manner of proceeding, with a constant reference to this nerve, I apprehend my patient was saved from much acute pain, and the nerve more safely divided, than at an earlier stage of the operation.

At several periods of this operation, the curved spatulas, used in my operation upon the *arteria innominata*, were found very useful, particularly in elevating the parotid gland, and keeping the tongue steady, whilst the tumour was separated from it.

Very little blood was lost during this operation. Two arteries only of any size were divided, the facial and lingual; and these only required the ligatures at the branch extremities; but each end was tied for safety. Another small artery behind, and a little underneath the posterior angle of the jaw, yielded some blood and was tied.

The flap of the cheek was now brought down, after waiting a few minutes to observe if any hemorrhage should come on, and secured in close apposition by three sutures, and adhesive straps. Lint, a compress, and the double-headed roller, completed the dressing. She was made as comfortable as possible upon the table, and directed to remain a few hours to recruit, and to be more convenient in case any hemorrhage should make it necessary to remove the dressings.

At eight o'clock in the evening, I found her removed to a bed, and in a comfortable situation. Some reaction of the circulation had taken place, but there had been no hemorrhage. The pain from the operation, she said, was less than she expected. For the first time, since the operation, she sipped three tea-spoon's full of cold water, and gave evidence, by a nod, that she could swallow. Directed one hundred drops of tinct. opii to be given, if any twitching, more pain, or restlessness, should supervene.

19th.—Seven o'clock, A. M.—Found her quite free from

fever and irritation, and, in every respect comfortable. Swallows cold water by the tea-spoon full with but little inconvenience. Did not take the tinct. opii last night. Slept several hours during the night.

Twelve o'clock, at noon.—Is comfortable; skin moist; pulse less frequent, and soft; directed an enema to be administered of soft-soap and water; has a little more difficulty in swallowing, but none in breathing.

Nine o'clock, P. M.—As well as in the morning. Enema operated three times, and relieved her. Pulse frequent, but not tense. She has taken about two ounces of cold water by the tea-spoon full since daylight.

20th.—Seven o'clock, A. M.—Had a very comfortable night. This morning, instead of nodding she answers "yes" and "no" to the several questions in an audible whisper.

Nine o'clock, P. M,—Much as in the morning.

21st.—Nine o'clock, A. M.—As comfortable as yesterday morning.

Nine o'clock, P. M.—No material alteration.

22d.—Nine o'clock, A. M.—Directed an enema to be administered as before. Allowed her to take, in addition to her cold water and teas, some thin chicken soup: is in every respect doing well.

Nine o'clock, P. M.—Tumefaction of the lips and cheek very trifling, not enough to effect the least change in the eyelids of the right eye.

23d.—Is in every respect comfortable.

24th.—Eleven o'clock, A. M.—Makes no complaint; dressed the wounds; union by adhesion has taken place in the whole extent, excepting about the ligatures and sutures. Suppuration having come on about two of the sutures, they were removed. Pulse about 120. Renewed the adhesive straps with lint interposed between them and the wound, and the double-headed roller.

25th.—Every way comfortable. Pulse 120.

26th.—Says she has no complaint to make. Pulse 80. Directed her to take a small dose of sulphate of magnesia.

27th.—Speaks audibly, and says she is very well. Pulse about 84.

28th.—As well as before; dressed the wounds; removed the two sutures at the upper part near the ear; wounds appear healed at every part, except where the ligatures remain upon the arteries. Pulse 80.

29th.—Feels very well; speaks distinctly; takes freely of soup and other thin food: pulse 100.

Dec. 3d.—Ligature from the carotid came away, and the other three ligatures from the upper wound. A small collection of matter was evacuated from under the integuments in the lower wound, which was produced by the irritation of the ligature.

4th.—Speaks and swallows very well; wounds just healed. Has used for some days a wash of spirits and water to the mouth, with a view to correct some fœtor of the saliva, and cleanse the mouth.

6th.—Found her dressed and sitting in an adjoining room, reading by the fire; looks and says she is very well. The bandages being all left off, the only deformity apparent is a little more tumefaction of the right cheek than the left; wounds just well; can move very readily the sound half of the under jaw. Permitted her to chew some animal food.

10th.—Wounds all healed—makes no complaint.

March, 1822.—To-day having visited her, I found scarcely any perceptible deformity. The right cheek appeared, upon close examination, to be a little more depressed than the left. I felt from within the mouth some osseous deposit to have commenced at the two situations at which the bone was divided. Her health in every respect is perfectly good, and she enjoys the free use of the left side of the lower jaw.

Nov. 5.—I have repeatedly heard of and seen the patient during the past season, and she continues to enjoy uninterrupted health.

25 Park-Place.

EXPLANATION OF THE PLATES.

No. I.

This drawing exhibits the appearance of the face before the operation was performed The rotundity of the right cheek will be observable, and the integuments below the under eyelid, on the stretch from the size of the osteo sarcoma.

No. II.

Represents the side of the face after the tumour was removed, and before the parts were closed.

a The part at which the jaw bone was divided near the chin.

b Condyloid process, where separated by the saw; the coronoid process being drawn up by the temporal muscle.

c The side of the tongue.

d Sublingual gland.

e Molar teeth of the right superior maxilla.

f Inferior portion of the parotid gland, with a plane of fibres of the masseter muscle turned up.

g g Portions of the common integuments.

No. III.

This figure exposes the appearance of the tumour upon its inner surface.

a Cuspidatus and first bicuspis teeth.

b Lower jaw where divided anteriorly.

c Bone at its posterior division.

d Furrow produced by the teeth of the upper jaw.

e e Two superior portions of the tumour covered by the membrane of the mouth.

f f The remainder of the inner surface of the tumour, dissected from the parts within the mouth.

No. IV.

The appearance of the patient after her recovery.

ART. II. OBSERVATIONS *on the Climate and Diseases of the Island of* CURACAO. *By* DANIEL L. M. PEIXOTTO, M. D. of New-York.

No writings of physicians, whether ancient or modern, have commanded more general attention, or secured more lasting regard, than those descriptive of the connexion existing between climate and disease. The cause of the value of such observations is obvious. They trace the most extensive and fruitful sources of disease, point out the most effectual methods of mitigating their force, or, perhaps, of wholly preventing their agency, and direct the choice of those who are forming new settlements, by warning them against such spots as experience has discovered to be unhealthy, and designating the signs of those that are friendly to human health and longevity. It were a needless task to cite the names of the authors who have distinguished themselves in this department, from the days of Hippocrates down to our own times, when a Chalmers, a Cleghorn, and a Fergusson, claim our admiration and respect.

Following, at an humble distance, the example of the great writers to whom I have alluded, I am induced to throw together a few observations collected during my residence in the island of Curacao. The facts they present will not be uninteresting, inasmuch as they relate to a place with which our commercial intercourse is frequent, and of which but little or no notice has hitherto been taken in a medical point of view; and, further, as they confirm various principles which have, of late years, deservedly attracted a great share of attention.

I know not how it has happened that the slight mention made of Curacao, by one or two authors of works on tropical climates, should convey the idea of its being a very unhealthy place, and of its climate being one of those distinguished for their unfriendliness to Europeans. That the island has, at times, been visited by epidemics, and been temporarily converted into an abode of disease, (and where is the spot, the most healthy, that has

not been thus afflicted?) that northern subjects on their first arrival in it, are exposed to those inconveniences which result partly from the change of climate, partly from the tropical situation of the place, I am not disposed to deny; but that it should be cited in conjunction with those places in the West Indies, whose localities, abstractedly considered, render them permanently unhealthy, neither experience will warrant, nor my regard for truth, suffer me to pass by without contradiction. Thus Lind, in one sweeping paragraph, denounces "the Dutch settlements at Surinam, St. Eustatia, and Curacao, as all being very unhealthy;"* and in a page immediately succeeding, he observes, "the Havana is not quite so unhealthy as Carthagena or Curacao." How an author, professedly writing a correct description of the different places in the West Indies, could have permitted himself to confound the dry, rocky, and proverbially healthy islands of St. Eustatia and Curacao, with the marshy situations of Carthagena, Havana, and, above all, with that of Surinam; the atmosphere of which is eternally moist, appears to me most surprising. Again, Moseley, in condemning the authority of Rouppe, a Dutch physician, whose means of information appear to have been limited, asserts of Curacao, that "its harbour is surrounded with marshes and stagnant waters."† This fact, we must presume, he borrowed from Rouppe himself, who had charge of a ship's crew at Curacao for two months, during which time they suffered much by disease. But the affections under which they laboured, of which Rouppe's description may be found in a note to Lind's work‡, do not seem to have been other than such as are indigenous to tropical latitudes, generally developed by causes existing on shipboard. In proof of this view, it will be seen by an examination of the description referred to, that the sickness prevalent on board of Rouppe's ship had made its appearance previously to her arrival at the har-

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* Lind on Hot Climates, page 86.

† Moseley on Tropical Diseases, p. 129—fourth edition.

‡ Lind on Hot Climates page 93.

bour of Curacao. "Die decimo nono portam Curacao intravimus, et *tunc* viginti ægrotaverunt." But even if the disease had broken out after the ship's arrival at the Island, as this event took place in August, which month, as well as September, is remarkable throughout the West India Islands for the prevalence of long continued calms, was there any coeval cause necessary to excite disease in newly arrived Europeans, confined on board of vessels of war, and that eighty years ago, when the subject of the health of seamen was imperfectly understood and little attended to? But, the cause, as has been shown, was altogether attributable to the general action of a hot climate, and not to any deleterious miasm arising from "marshes and stagnant waters." Indeed, there is not a swamp to be found in the whole island, nor any stagnant water, even in rainy seasons, for several miles from the harbour; the shores of which are chiefly table-rock, washed by the tide of the ocean, and this circumstance moreover obviates any objection arising from the consideration that places are known to undergo many variations in their localities in a period of years. I have been the more particular on this subject as it has a close relation to a principle of great application in the investigation of tropical diseases.

Curacao is situated in latitude 12° 8′ N. and longitude 69° W. distant twenty leagues from the coast of South America, of which the highlands are plainly discernible in clear and calm weather. The island extends from the N. W. to S. E. in length forty miles, and its mean breadth may be fifteen. The general appearance of the island is barren and parched, its surface broken into abrupt masses of hill, which raise their picturesque forms in every direction. They are, however, bare of all marks of vegetation or verdure; cultivation being confined to the vales in which the fruit and maize plantations are more advantageously located.

The soil is superficial, consisting of a light mould covering a rocky substratum, in which gypsum, pumice, and ochre, and some argillaceous earth are found. Water is to be had by digging into the rock; but there are no rivulets of fresh water,

nor any marshes to be seen in any part of the island. The hills are of no inconsiderable height, some of them having an elevation of from ten to fifteen hundred feet above the level of the sea. They no doubt serve a highly useful purpose, by collecting and conducting the currents of air along the valleys; and adding to their force and impetus, they refresh the atmosphere. In one of these hills on the northern part of the island, there is a highly interesting cave, comprising several spacious and romantic apartments and vaults, from the walls and roofs of which the most beautiful stalactites are pendent, while in the floored recesses there is water of the purest kind.

The air is pure and dry; seldom, or never, darkened with those mists and fogs which enwrap us so frequently further to the north. Clouds are constantly driven over the island by the brisk trade winds that prevail throughout the year, excepting in August and September. They are highly favourable to the reduction of the extreme solar heat, by the constant succession of grateful shade which they lend to the earth.

It is a circumstance worthy of note, that fires are with difficulty lighted in this island, and unless assiduously fed, are instantaneously extinguished. Hence the carelessness of stupid negroes, who expose lighted coals near to plank floors, the frequent, and often lamentable source, in other islands, of the most ruinous conflagrations, escapes here with impunity. To what causes can this be owing?

Metals oxidate rapidly after exposure. The most polished steel soon loses its lustre, and assumes the dingy hue of rust. What circumstances they are that occasion this in an atmosphere so dry as that of Curacao, does not at first clearly appear. Cleghorn* observed the same phenomenon at Minorca, and imputes it to the moisture arising from the spray of the sea, that in windy weather is driven over the whole island, as evinced by the brine found on the leaves of vegetables growing in parts the most remote from the ocean. Our more sa-

* Diseases of Minorca, p. 1.

gacious countryman, Chalmers, assigns as a cause for the rapid oxidation of metals in Charleston, South Carolina, the existence of an acid in the atmosphere exhaled from the surface of the rich and moist earth.* But it does not appear that his view of the origin or nature of this acid was very clear or satisfactory. That moisture is not sufficient to produce this effect must be evident; for if it were, the oxidation of polished metals should be rapid in direct proportion to the dampness of the soil and the humidity of the atmosphere: but this is by no means the case. Bontius† asserts, that at Java metals acquire rust sooner in the driest season of the year than they do in the dampest seasons in any part of Europe. This conforms with my observations at Curacao; in which the air is constantly dry, and the rust is acquired by metals much sooner than it is in New York in our most wet seasons. I should therefore be inclined, with Chalmers, to call in the aid of an acid not arising from the soil, but furnished by the decomposition of the muriate of soda, disseminated either by means of the spray of the sea, as is believed by some, or as has been maintained by others, by exhalations from the sea itself. That the atmosphere in the neighbourhood of the sea is impregnated with salt, does not admit of a doubt.‡

The constant evaporation by heat of the salt thus carried into the atmosphere precipitates, according to an experiment by J. Murray of London, free muriatic acid. If this interesting position be admitted, we have a satisfactory solution of the difficulty, and the rationale of the oxidation of metals in dry situations near the sea, is not problematical. But whether this solution be admitted or not, the acid principle evidently acquired from the sea must still be resorted to. May not this acid have an agency in the rapid extinction of fires before described?

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* Chalmers on Diseases of South Carolina, v i. p. 11.

† Moseley, p. 100. Note.

‡ For some highly interesting facts on this subject, I cannot do better than refer to a paper on Salt Storms, published by my friend, Dr. John B. Beck, in Silliman's Journal, vol. i. No. 2.

The temperature, as observed by Fahrenheit's thermometer, in the shade, ranges in the course of the year from 76 to 90; the ordinary heat being 85. The seasons differ less in this, than in the larger islands to the north, neither do the rains recur at regular periods as in most other tropical situations, being frequently wanting altogether for a succession of seasons. This was especially the case for the years 1820, 21. During all this period there has been scarcely any benefit of rain felt, except from a few occasional showers, and occurring at protracted intervals, and consequently, a total failure of the crops has been experienced, to the great loss of the planters. With the exception of a few fruit trees, to which drought is favourable, there was no appearance of vegetation any where to be seen. The leafless desolation of winter reigned under a tropical sun; ordinary wells were exhausted; animate and inanimate nature suffered under the burden, and seemed ready to waste away and perish. Domestic animals, particularly, suffered for the want of herbage and water. In one plantation six hundred out of one thousand sheep perished. Nevertheless the inhabitants experienced little or no inconvenience in their persons, neither was there any increase of mortality, or sickness, observable, not even among strangers who visited the island. To the constant perflation by the N. E. winds, which, in these years, were scarcely observed to cease even in the calm months, as well as to the absence of "marshes and stagnant waters," that would elsewhere, during the process of evaporation and drying, have exhaled the most deleterious effluvia, must be, in the main, attributed this escape from disease. The same immunity of the inhabitants of places suffering under great drought has been frequently observed elsewhere. Among other instances, Chalmers records, that during the year 1752, the inhabitants of Charleston suffered dreadfully from oppressive heat and drought, but never was there a more healthy season known, "so long as the weather continued steadily warm and fair." These facts accord with a law laid down by that faithful and accurate observer Rush, which is, that the same "state of the atmosphere, whether cold or warm, moist or dry, con-

tinued for a long time without any material changes, is always healthy."*

Hurricanes are very rare in this island. Since the severe one of the 17th of October, 1807, there has been none experienced.

The city of Curacao is built on the S. E. part of the island, on a narrow slip of land, washed on one side by the sea, and on the other by the arm of a bay, which forms the harbour, and stretches far inland. It is surrounded by a wall and bastions on three sides, while its commodious quay is open to the harbour in front. The streets are paved, and, with the exception of one, are narrow. The houses are spacious and lofty, and thicky piled upon each other. I have been induced to ascribe to this circumstance of the streets being narrow, the superior healthiness of the crowded population. The height of the houses, and their proximity to each other, prevent the sun's rays from heating the circumambient air to that oppressive degree, which is so insupportable in places in which wider streets and more ample spaces give free scope to the torrid sun to exert his energies in full play. The narrow streets, moreover, attract the breezes, and direct them, with additional impulse, upon the verandas fronting each house.

The rest of the island is composed of the suburbs of the city, and of plantations. In these, Guinea corn and the ordinary tropical fruits grow in the highest perfection. The medlar, the mango, orange, lime, fig, banana, tamarind, and cocoa, are among the principal of the many and delicious fruits that in gay confusion enrich this barren soil, and administer to the simple luxury of the inhabitants.

There is nothing very particularly remarkable in the manners of the people. They are in general characterized for sobriety and early hours, and they live to the same good old age which is enjoyed in the most favoured regions of the globe. The females menstruate early, and there are not wanting seve-

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* Rush's Account of the Climate of Pennsylvania.

ral instances of the menses not ceasing until after fifty. The secluded habits of this sex, the relaxing influence of the climate, and, among the coloured portion, (of course the most numerous,) the too early indulgence in sensuality, predispose them to chlorosis, amenorrhœa and leucorrhœa, diseases which are very common, and frequently resist all ordinary methods of cure. They become mothers early, and bear pregnancy and parturition with an ease and nonchalance that would half justify Byron's exclamation:

> "Who round the North for paler dames would seek?
> "How poor their forms appear, how languid, wan, and weak!"

It has already been observed that Curacao is proverbial for health. The absence of deleterious causes of disease, the pure and dry air, the old age to which the inhabitants reach, all testify to this fact. To it invalids from the neighbouring islands frequently resort, and with decided benefit. Convalescents from the endemic fevers of St. Domingo and Jamaica find immediate relief on their arrival in Curacao; and the most obstinate cases of tertian and remittent fevers, here frequently yield to the salutary impressions of a healthful air. Even broken-down constitutions, the sure, and, in hot climates, speedy attendants on dissipated habits, are here refitted, or, at least, enabled to bear "life's burden" with comparative ease.

Phthisical patients are, however, seldom benefited by a stay at this island. The dry constitution of the air seems to be rather injurious to those labouring under pulmonary affections. The acidity, too, with which we have supposed this atmosphere to be impregnated, adds no doubt to this injurious disposition. The asthmatic likewise find this place unkindly to their affections. Of this I had proofs in my own case. When a child, I laboured under this complaint here, and it was not until I changed climate that I found relief. On my revisit to the island of late I had a recurrence of the complaint, and suffered several violent paroxysms.

Among the endemial diseases of this place may be first mentioned those affecting the eye. Weak sight is a very general complaint among the inhabitants, even in those that are young;

and hence the use of glasses is very common. The more serious affections of this organ to be met with are ophthalmies, pterygia, cataracts, and amaurosis.

The causes of the prevalence of these disorders are not unworthy our notice. The rays of a vertical sun darting on a barren soil, over which verdure seldom or never throws those refreshing hues on which the eye delights, in more fertile regions, to repose, are reflected with a dazzling intensity which proves irritably painful to the eye; and inducing in that delicate organ an indirect debility, powerfully predisposes it to be excited to disease. So the sterile plains of Egypt, for the same reason, have long been noted for their unfriendliness to the vision of the natives. But in Curacao the white lime, with which all the buildings, both public and private, were formerly washed, has been a more actively exciting cause of these disorders. On this account it was that the government issued a decree forbidding houses to be covered with any colour lighter than yellow; since the operation of which the inhabitants are said to suffer less. The same cause has been observed to produce similar effects at Bologna, in Italy, and at the isle of Malta.* If it were necessary to enter more into detail, the neighbourhood of the sea, the occasional prevalence of measles, and, in former years, of the small-pox, might be cited as additional causes of the many defects and diseases of the eye prevalent in this place.

Hernias are frequent, particularly among the negroes, whose skill in reducing them is very great. Fistulæ, particularly in ano, subsequent to piles, are often met with, and seem to be connected with the bilious disposition so incidental to those living in warm latitudes.

I could not discover that any case of hydrophobia had ever occurred here. The disease is generally known by name only.

In particular months of the year the inhabitants are exposed to be poisoned by certain fishes; more particularly the yellow

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* Assalini on the Plague, p. 126.

bill sprat. This poison does not appear to be a permanent property of the fish, as they are eaten with impunity in general throughout the year, and it is at times only that they become possessed of this deadly venom. Whoever becomes the unfortunate victim of the unsuspected poison, is either hurried off by immediate and excruciating convulsions, or he suffers and lingers with violent symptoms of a bilious cholera, till death comes to his relief. Sometimes the poison is eliminated and counteracted by means of emetics and subsequent stimulants; but the very few cases that fell under my own observation proved speedily fatal.

The measles occasionally reign here epidemically, and they commit great ravages. In the early part of 1821 they made their appearance, and continued until the middle of June, proving very fatal, particularly to children. They were in most cases attended with inflammatory symptoms, and the secondary fever frequently assumed a malignant type, or went off with a diarrhœa, which proved in the sequel obstinate and difficult of cure. The fatality of this disease was owing, in most cases, to a neglect of the antiphlogistic regimen and treatment; alexipharmics and confinement being frequently resorted to in consequence of the ignorant fears of the blacks, and the far more unpardonable ignorance of the empirics. On the other hand, early depletion by the lancet, or, where the case was less violent, cathartics, and a strict conformance to rules of abstinence, and the free admission of cool air, were found successful in allaying the violence of the disease, and preventing those chronic sequelæ, usually incident to this complaint. Candour, however, requires the confession, that the same treatment in various members of the same family was not always attended with desired success, and that where no visible difference in the cause or symptoms of the disease could account for such variety of issue. The epidemic was observed by the physicians generally to assume its most violent aspect some time after its first appearance, and then gradually to abate until its final extinction.

The dysentery may next be mentioned, and as occurring more frequently among the blacks. I did not observe any symptom, as rendering it worthy of particular description. The acute form of this disease, however, frequently ended in chronic diarrhœa, which, unless properly managed, frequently wastes the strength of the patient, and finally destroys him. The treatment from which I experienced the greatest, and often most decided benefit, was to deplete or vomit the patient in the earlier stages, according to the degree of inflammatory symptoms present, and the condition of the stomach, as indicated by the tongue, &c. After this, my dependence throughout the disease was placed upon calomel, combined with rhubarb or opium, according to circumstances. A mild laxative, as castor oil, was occasionally interposed, and with much benefit. If the disease had assumed its chronic form, I added the infusion of some light bitters, among which camomile was the most grateful. In the leeward harbours of the South American coast, this excellent tonic is deemed a specific in the treatment of the disease, particularly when combined with milk.

I shall next treat of fevers, premising a few general remarks that will apply in common to all of them.

It has been too much the fashion to refer to the exclusive agency of bile, every disease which flourishes in hot weather and climates, and, under this impression, to include them all under the broad and vague appellation of bilious. It was thus formerly with putrefaction, with which all tropical diseases were necessarily supposed to be connected, and hence the term putrid, superadded to their frequently unmeaning list of titles. Moseley has sufficiently exposed the fallacy of this doctrine, and shown the dangerous tendency it produced on supposed and gratuitous curative indications. The error, as regards bile, is no less glaring, and no less productive of danger. The indiscriminate use of remedies calculated to act on the liver, to the absolute neglect of the more prompt means requisite to arrest the progress and allay the violence of inflammatory action in more vital organs, (to which affections robust, plethoric new-comers, are most subject on their *first*

arrival,) has been the frequent and fatal effect of this unphilosophical spirit of generalization applied to the diseases of warm latitudes.

Bilious symptoms are always present, to a greater or less degree, in the fevers of the tropics. The sympathy subsisting between the liver and skin, induces in the functions of the former, a similarity of affection with that, which those of the latter never fail to undergo, under the operation of continued and high degrees of heat. Hence this important viscus is seldom or never in a perfectly sound state, even in those who are in seeming possession of health. But no sooner does the frame succumb to disease, be it of whatever kind, than the biliary organ is among the first to experience the effect, and, in many cases, it forms the most prominent point of attack.* This is more especially the case in the ordinary fevers of the climate to which the epithet bilious, is, in general, applied with strict propriety. But frequently these morbid affections are alleged as causes, or as constituting the diagnostic symptoms of diseases in which they are, in fact, no more than accessary or incidental occurrences. Thus yellow fever, it has been maintained, is a bilious disorder, because bilious symptoms are occasionally present during the disease, and post-mortem examinations generally show some affection of the liver to have preceded death. But, from what has been said, it must appear that the conclusion drawn from these circumstances occurring in yellow fever, must, to say the least of it, involve no small share of doubt. When we further consider that in this disease other organs invariably present a far greater degree of lesion, both in the symptoms of the disease and on dissection, this doubt will amount to a certainty that the yellow fever is not a bilious disease.

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* In hot climates a sound state of the liver is never to be expected after death, whether the disease has been acute or chronical; of the latter class of disease it is almost always the seat and origin.—Moseley, p. 443.

The bilious fevers, then, are those in which the liver is the origin of the disease, and bears the onus of suffering in common with the viscera subservient to it, as evinced by the most marked and uniform symptoms of the disease. The obvious indications, and the method of cure, which, when early attempted, is generally successful, consisting of emetics, cathartics, diaphoretics, and mercury, all tend to the same conclusion. The creoles and the seasoned Europeans and Americans are equally obnoxious to their attacks; those being especially their prey who indulge in an abuse of the non-naturals. Many pay a dear annual tribute to these fevers; frequently making hair-breadth escapes, from the shock of which they no sooner recover, than they undauntedly resume their habitual course, till it at last brings them to the goal of both their pleasures and their pains. These subjects generally carry with them some mark by which they may be distinguished, a chronic ulcer, the piles, fistula, or, to top the climax by the severest of all ills to 'which flesh is heir to,' dyspepsia, and, its faithful Achates, hypochondriasis.

The natives are more particularly subject to catarrhal, nervous, and typhoid fevers. I had no opportunity of seeing these, except as they occurred in solitary instances, and these were mostly cases of children, in whom the causes of disease were those indigenous to all places in every climate.

Nervous fevers are, however, as I am informed, epidemical at times, especially after a season of rain, followed by intense heat and calms, when many of those, who, during the reign of the ordinary healthy seasons, were accustomed, with impunity, to indulge ad libitum, now pay the forfeit of their sins against the laws of temperance and sobriety. In the cure of these fevers, after properly premised evacuations, the bark and snake-root are early admissible.

Intermittents are seldom seen in Curacao. The observation made by Hillary on Barbadoes will apply to this island with strict accuracy. "Intermitting fevers," observes that author, "especially tertians and quartans, are very rarely or never seen in this island, unless they are brought from some of

the leeward islands, or some other places less cultivated, and not cleared of wood."* A similar observation on Savanna la Mar, in Jamaica, was made by Jackson.† In Curacao they are occasionally brought from the States, and from the large islands and harbours on the leeward coasts, and, as I before remarked, they are speedily subdued. But although the intermittent is not native here in general, exceptions do occur, and some have fallen under my observation. I attended, at different periods, two negroes from the plantations, who were affected, the one with a quotidian, the other with a tertian type of intermittent fever. These negroes, as I was correctly informed, had been engaged in labour in low places, near to which I found, on examination, ponds of stagnant water had been temporarily formed by partial, but, as is usual within the tropics, heavy showers of rain. This fact strikingly illustrates a principle formerly not well understood, that places which are in general remarkable for their salubrity may, by the operation of transient causes, change their character, and become, for a season, unhealthy. Thus situations that are ordinarily dry may, after the formation of heavy rains, or by the overflowing of rivers of fresh water, of pools, standing waters, or temporary marshes, change features and localities with those that are habitually moist; while these latter, by the agency of the same causes, may become, for a while, free from their usual causes of disease. The observation made by Volney in Egypt, that "a boundless plain, at different seasons, is an ocean of fresh water, a miry morass, a verdant field, or a dusty desert," beautifully exemplifies the gradual change which the same district of land undergoes in its aspect and medico-topographical character, and accounts for the varying degrees of healthiness and unhealthiness that give rise to opposite conclusions drawn from observations made at different periods.

To return. I could not entertain a doubt as to the peculiar

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* Rush's Hillary, page 21.

† Jackson on Fevers, page 5.

cause that excited the fever in the cases which I have recorded, because there was no other case of the disease in the island; and secondly, because a removal of my patients to the dry seashore lent almost instantaneous efficacy to the ordinary medicines employed to arrest the disease.

It has been questioned whether typhus fever can or does prevail excepting in cold weather. That warm weather is equally or more favourable to its production and extension, cannot, however, be doubted, after the mass of testimony extant in medical records, if we begin with Pringle and continue our examination through the later and more able authors who have written on typhus since his time. I shall not dwell on this point, but proceed to observe, that if it were true that hot weather was a certain preventive of this fever, it should follow, as a regular consequence, that it could never prevail in the torrid zone. Is this, however, the fact? If we examine the writings of Thomas, Mc Arthur, Ferguson, and all English physicians who must be supposed to have been well acquainted with the characteristics of typhus, we shall find that they have given their unequivocal testimony in favour of the prevalence of this disease in the West Indies. Their testimony on this subject is the more invaluable, as they do not appear to have been biassed by any theory in favour of either side of the disputed question; of which dispute it is more than probable they did not dream. The following brief account will tend to show that it can originate under the fervor of the vertical sun, and be imported into an island suffering under heat rendered more intense by a long state of drought.

In July, 1821, an extensive emigration took place from the coasts of Venezuela. The success of the Colombian forces compelled those Spaniards, who were unwilling to abjure their faith to the mother country, to desert their homes at a moment's warning, and, embarking in crowds on board of the first vessels they could reach, to disperse themselves over the neighbouring islands. On board of these vessels they were, of course, ill accommodated; and during their passage they suffered immeasurably from the privations to which they were

necessarily subjected, in the absence of every thing like comfort or cleanliness, and, above all, from the depression of spirits, which their situation and the gloomy prospects before them could not fail to inspire. Nearly two thousand of these arrived at Curacao, where many were compelled to crowd together in small habitations, destitute, in many instances, of every convenience. Many of these emigrants likewise suffered much by the change of water, which was, at this time, owing to the long continued drought, both scarce and not always of the best quality. They had not been long in the island before typhus fever appeared here and there among them, and it proved in some instances very fatal, but in the greater part less violent and more amenable to the aid of medicine. I observed that most of those who were attacked had come from places on the main, in which the remittent and typhus were endemic, as Porto Cabello, and places in the interior, &c. and I was, on account of this circumstance, led to conclude, that the subjects of the disease must have received the seeds of it before their departure from their homes, and that the depressing influence of the calamities of war, with the other exciting causes of low fever to which they were subsequently exposed, had proved the means of developing the disease. In these cases, the same loss of sensorial power and depression of nervous energy, which is observed in the typhus of authors, the same pungent heat and invincible stricture on the surface; the same quick, low, and irritable pulse; the black and parched tongue, tremulous too withal on exposure; the sordes about the gums and lips; the dull eye; the locked up state of the secretions and excretions; the exacerbation at night; and towards the close of the disease, the low delirium and subsultus tendinum; fœtid and involuntary discharges, petechiæ, &c. were to be observed; and when thus combined, ended in the death of the patient from the eighth to the eleventh day. In cases where the disease terminated favourably, its course was protracted for several weeks. Dysenteric symptoms were occasionally joined to the fever, and in many instances pulmonary irritation was very constant, and added to the restless torments of the sufferer. The treat-

ment was such as has elsewhere been resorted to in typhus gravior, making allowance for the climate and the peculiar condition of the sufferers. Bleeding was in no instance resorted to, but copious evacuations from the bowels, gentle diaphoretics, and blisters, in the early stages, followed in the further progress of the disease, by the use of stimulants and opiates, according to circumstances, attention to the alvine discharges not being omitted, were ever found highly beneficial. On the other hand, a blind neglect of the evacuating system, and the too early recourse had to stimulants, by untimely fears of debility and putrefaction, never failed of doing harm, and generally betrayed itself by the subsequent more direful symptoms. I cannot omit adding, that contagion did not in the least appear to be a property of the fever, all those affected having been equally subjected to the same causes, and in no instance was the disease communicated to others. So that after a short period it ceased altogether.

The last disease of which I shall have occasion to speak, is the yellow fever. To pass over in silence a subject so interesting to his country would, in an American physician, be as culpable, as it would be unpardonable, on the other hand, to tire his reader with a repetition of stale and common-place details. Undeterred, then, by scruples which would urge the impropriety of describing a disease so often and happily described before, I shall furnish a concise enumeration of its most remarkable symptoms, as they fell under my observation at Curacao. The impression made on my mind by the first case to which I was called, can never be erased ; the peculiar physiognomy, the insidious progress, the acute mental and bodily suffering, and the rapid and fatal issue, furnished a bedside lesson of disease, which not the ablest and most pointed descriptions can ever convey. The occurrence of this disease too in an island so dry and free from many of the sources of the disease, on which too much reliance has been placed, will not be deemed unworthy of record.

The yellow fever is endemial to this island at all times and seasons, but it occurs more especially in the calm and sultry

months of the year. New comers from northern latitudes are its appropriate subjects; the natives and seasoned inhabitants being exempt from its attacks. When a great number of the first description of persons arrive at the same time, as sailors on board of vessels of war, or troops in transports, the disease generally prevails to some extent among them, and is then considere depidemical. Solitary individuals, arriving from time to time form the sporadic cases, which are more or less occurring during every month of the year. I had an opportunity in 1818, of seeing the disease in its epidemic form on board of two Dutch vessels of war.* Sporadic cases never failed to present themselves, at all times, during my stay in the island.

The disease generally invades the patient with the ordinary symptoms of fever: slight rigors, never amounting to a chill; pain in the head and loins; uneasy sensation about the stomach, and a general sense of weariness. Sometimes these symptoms are altogether wanting, and pyrexia seizes the patient suddenly: at other times they continue for several hours, and then are succeeded by increased arterial action, quick, full, and chorded pulse; great, and even pungent heat of skin; increase of the pain of the head, more particularly across the eye-balls; soreness and pain in the loins, stretching forward to the umbilicus, or shooting down to the thighs and settling in the calves of the leg, in which the patient likewise complains frequently of a sense of weakness. The countenance has a sad and characteristic appearance: the eyes are inflamed and watery; the tongue is slightly covered with a whitish mucus, and is most frequently moist. The skin is sometimes of a dingy hue, mostly not altered in its appearance, and frequently it is covered with a slight moisture. The stomach is very uneasy, experiencing a sense of vacuity, some-

* I was at that time a student of medicine, and was indebted for much and highly valuable information to my respectable friend Dr. Cabrera, a citizen of Venezuela, alike distinguished for his professional skill, and for the eminent devotion he bears to the rising liberties of his country.

times of hunger, and is generally painful on pressure from without; in some cases it retches even at this early period, and if vomiting be superadded, the prognostic is fearfully certain. The bowels are costive, and respond with difficulty to the action of purgatives. The urine is scanty, and what is voided is reddish and clear. The mind in general is serene, but fearful of the event.

These symptoms continue increasing for forty-eight or sixty hours, when relief seems approaching in a temporary alleviation of the more violent symptoms. The febrile heat and pain yield and frequently go off altogether. But when the stomach has once been disordered, there is no cessation of its sufferings. This second stage has not inaptly been termed, by Moseley, *Metaptosis;* it is the token that re-action is exhausted, and the system is undergoing a rapid change, from a state of excitement to one of collapse and passive suffering. The third or last stage accordingly developes itself sooner or later in the sunk and irritable pulse, though in some cases it is little changed; the low temperature of the body; the approaching coldness of the extremities, and yellowish suffusion appearing first about the eyes and angles of the mouth, and on blistered surfaces, particularly in moments when the patient, either from vomiting or any other exertion is exhausted, then diffusing itself over the neck and breast and whole body. The patient complains now of increasing uneasiness about his stomach, and is urgent for cold drink, which is no sooner taken than it is rejected, and accompanied with increasing quantities of black membranaceous flocculi swimming in the fluid, and of a slate-coloured sediment at the bottom. He is frequently free from all disturbance, and inclines to doze, but he no sooner falls into a slumber, than the perturbation of his stomach becomes perceptible, and vomiting is brought on. He now lies on his back, with his knees drawn up in the bed, and is intolerant of any covering about his head: he occasionally sighs, and tosses his hands to and fro; is anxious for something hard upon which to press, and frequently leaves his bed with insane violence, and grasps the floor or the bed, and seems to experience relief

from the indulgence. Hiccough, increase of black vomiting, dark and fetid stools, hemorrhage, and perhaps a convulsive fit end his acute sufferings.

Such is the general outline of the train of phenomena characterizing this disease, whose mortal progress it is the severe duty of the humane physician to watch with scarce a hope of any other reward to his labours and anxiety except an addition to his melancholy experience.

All cases are not, however, thus distinctly marked. Patients sometimes labour for a day or two under no other symptoms than those attending an ordinary cold, for which a dose of salts from the medicine chest is prescribed; their eyes are watery, they feel disinclined to move about, and lie down listless and unconcerned of what is passing about them, when, of a sudden, retching and vomiting come on, and perhaps before the physician has time to appear they are in the agonies of death.

On the treatment I have nothing new to offer, and shall not say any thing. Emetics were, however, proscribed, and blisters were by some thought hurtful. In two cases, which threatened to put on the most violent form of the disease, the most active depletion by the lancet, and brisk cathartics led me to flatter myself that I succeeded in arresting the disease.

1. As to the character of the fever, it is evidently an ardent continued fever that vents itself on the stomach and brain, and originates only in places near to the sea. It is distinct from the bilious remitting fever that arises in hot weather, in warm climates, and in every situation that is exposed to the action of heat and moisture. These diseases have been confounded, it is true, but they are characterized by so many differing circumstances, that it appears very strange how they could have been ever identified. In the yellow fever the liver is not the cause of the disease, nor is it primarily affected, as is the case in bilious fevers;* there is no remission of the fever as in

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* In addition to what I have said in a preceding page on this subject, I may here add that a further confirmation is derived of the liver not

the bilious remittent, the abatement of the excitement which takes place in the metaptosis being no other than, as it has been accurately described by Lining, a total cessation of the fever. To the yellow fever, the newly arrived northern man is alone subject, whilst the native and seasoned inhabitants are alike, with the former, obnoxious to the bilious fever. The yellow fever attacks the same person once only, excepting in cases where a temporary residence in a cold country has, in the interim, predisposed the constitution anew to an attack of the disease,* whereas the West Indians are liable to repeated, and even annual attacks of their bilious fevers. The treatment too, differs in the two diseases: emetics, which are highly proper in the bilious remittent fever, being most generally fatal in yellow fever. On each of these highly important heads, abundance of testimony could be adduced, but the limits to which I have prescribed myself will barely permit me to refer to the pages of Moseley, Jackson, Lining, and Mc Arthur, from whose ample experience the above results and conclusions have been drawn. But the locale of the two diseases differs, and this leads me to notice,

2. The cause of yellow fever. I am aware that I am treading on dangerous ground, when I approach this subject. The immediate cause of yellow fever is far from being known; but does not this very circumstance distinguish it sufficiently from the bilious remittent, whose source is indubitably heat and marsh miasma? That heat is no less necessary to the production of yellow fever, cannot be denied; but it is by no means apparent that miasma from marshes is a cause of the fever. For if it were, the yellow fever would be co-extensive with the bilious remittent, which is by no means the case. The latter pre-

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being necessarily affected in yellow fever, from the circumstance that in dissections of this disease in the United States the liver is seldom or never found diseased.—See *Dissections*, by Drs. Physic and Cathrall of Philadelphia in 1793.

*It is the predisposition thus newly acquired by the inhabitants of northern latitudes, during their winter, that exposes them to repeated attacks of yellow fever.

vails under the influence of heat and moisture in the interior as well on the sea-coast of those countries in which it is endemic. But the yellow fever is confined to the neighbourhood of the sea; in the inland districts of the larger islands, and of the American continents, it is unknown. This difficulty it has been attempted to get over by the assertion, that they are various grades of the same disease. But from the locale assigned to each, it must be evident that the cause is entirely different, and hence the logical inference is just, that the effects are likewise different. Besides, there is a sufficient number of facts on record to prove that this fever occurs in hot latitudes at sea, before new-comers have approached the land;* and hence it has been considered in these cases a disease of pure excitement from heat alone. The occurrence of the disease in perfectly dry situations, as at Curacao, tends to the same point; nor does the prevalence of the disease as an epidemic, detract from the force of this conclusion, as it implies a difference in the number only of those simultaneously exposed and attacked, not in the nature of the cause or origin of their disease. The knowledge which we possess on this subject at this day resolves itself into the following data: that yellow fever prevails in tropical and tropicoid climates, and among the agents necessary to its production, the presence of heat and the neighbourhood of a sea atmosphere are alone well defined and understood.

3. If I approached the cause of yellow fever with fear, I must speak with delicacy of its *contagious* property. In the cases which I have seen of this disease, I never could perceive the existence of this quality: sporadic cases were certainly free from it under all circumstances, and the epidemical cases which have been suspected to possess this virus, were only observed to impart it within the sphere of the original source of the disease. But there are so many contradictory facts recorded on this head, and my experience with regard to it is so limited, that it would be daring indeed on my part to offer any

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* Lind, M'Arthur, &c.

conclusion. Happy shall be his destiny, to whom it is reserved to analyze and trace the causes which have involved this subject in so much mystery and discussion; and more happy he, who can reconcile the jarring opinions and discrepant views of such men as Bancroft, Chisholm, Rush, Miller, and Hosack.

ART. III. *A case of* COMPOUND PREGNANCY, *accompanied with prolapsus Uteri. By* MARTIN L. BRYAN, M. D. Fairfield, Herkimer County, State of New-York.

MRS. C. N. previous to the age of twenty, had been accustomed to labour in the field, and was uncommonly robust and hardy. About that time her health began to decline; menstruation was performed with less regularity; the quantity of the secretion was diminished, and a leucorrhœal discharge was observed, particularly about the menstrual period. At length, as she was carrying a heavy burden, she "felt something give way," and was immediately seized with excruciating pains in the back and hips, an uneasy sensation in the groins, and difficulty of voiding urine, with cough and pain in the side, accompanied by fever. The parts of generation, nates, and thighs, soon became inflamed and very tumid. The fever continued for several weeks, and the inflammation and swelling for months. The attending physician bled her frequently, and used cooling applications; but, as she concealed from him the seat of her greatest distress, no examination was proposed. After about a year, she recovered so as to be able to walk, by inclining her body a little forward, though she was still liable to a return of the inflammation and swelling, from any considerable exertion.

About three years after this attack she was married. The expectations of her husband seemed disappointed, and his enjoyment incomplete; and the same misfortune which was to him a cause of discontent and dissatisfaction, proved to her a

source of pain and disease. In this situation of things, in March, 1818, about three months after the marriage, I was consulted in her case. At that time I received from her the foregoing account. She expressed the greatest anxiety for relief, and preferred death to a life so miserable. I immediately proposed an examination, to which she readily consented. I found the uterus apparently enlarged, and filling almost the whole cavity of the pelvis. The os uteri was resting on the perinæum just behind the fossa navicularis. The vagina was folded, thickened, and almost as hard as the walls of the uterus. I could not introduce my finger, in any direction, more than one inch and a half. The parts were sensible to the touch, and she complained of pain during the examination. I endeavoured to elevate the uterus, but found it completely fixed in its situation. Having directed her to keep her bowels loose, and apply cold water freely to the parts, I left her in a state bordering on despair.

She continued subject to frequent attacks of inflammation and excoriation of the vulva and surrounding parts. In February, 1820, during a suppression of the menses from cold, she had an attack so severe as seriously to threaten her life. From this, however, she partially recovered, and in April following, the menses returned. About this time she became pregnant, and her health improved. Although the symptoms of pregnancy were more troublesome in her case than I have usually found them, yet for three months she continued to be more comfortable than she had been for some time previous to impregnation. I suggested her probable situation; but she refused to listen to the suggestion, supposing it impossible for a woman in her situation ever to conceive. At the commencement of the fourth month, her afflictions seemed to return with increased severity. In the fifth month the symptoms became alarming. Fever, swelling of the labia, thighs, and hips, soreness in the epigastric region, and pain in the back, accompanied with a train of nervous symptoms, seemed to indicate that the necessary rising of the uterus was breaking up the connexions, which had retained that viscus in its unnatural situation.

From this time she was unable to walk, till about the end of the seventh month.

On the 14th of December, 1820, I was called at five o'clock, A. M. to attend her in parturition. She had passed several very restless nights, her abdomen was unusually distended, and she felt a constant pain in her back and bowels. There seemed to be a tonic action of the uterus, though there was no appearance of regular labour pains. I let blood, directed her bowels to be freed by an enema, and left her. I returned about three o'clock, P. M. and found her very restless. The pains had become almost insupportable, but were still irregular. On examination, I found the os uteri considerably dilated, and the feet of a child presenting, with the toes towards the right side. I waited at least an hour, in expectation that the pains would become more regular, and the feet descend without assistance. The os uteri was dilated, but the uterus acted feebly on the child. With the left hand I brought down the feet, and, as the thighs were passing, by gently turning them, I brought the face in the direction of the right sacro-iliac junction, and she was soon delivered of a male child.

As there was sufficient evidence that another child was contained in the uterus, two ligatures were passed around the cord. The child being removed, I introduced my finger into the vagina, and found another set of membranes, through which a child's head could be distinctly felt, in the second presentation of Baudelocque. Though the pains were feeble, she was delivered of a daughter, in about two hours. The abdomen still seemed to be considerably enlarged, and, on examination, I found the membranes of a third child, which presented its left shoulder to the os uteri, its head resting in the right iliac fossa, and its back being towards the anterior part of the uterus. At this time my patient seemed almost exhausted. After administering cordials, I cautiously introduced my left hand into the uterus, and succeeded in carrying up the shoulders and bringing down the feet. The uterus now seemed incapable of making any effort to expel the child. I directed an assistant to keep up friction with spirits over the

region of the uterus, and to make pressure on its fundus when there were appearances of pain. By these means, and the cautious use of extracting force, she was soon delivered of the third, a male child. The umbilical cord being tied as before, I passed my finger into the vagina, and was astonished to find another set of membranes, through which I felt the knees of a child, with the thighs towards the mother's right side. As my patient seemed sinking, I directed volatiles to be applied to the nose, and gave her a little wine and water. To wait for nature to accomplish the delivery, would evidently be attended with danger. I introduced my hand and brought down the feet. I waited a short time. There was scarcely the slightest appearance of pain. Friction with spirits, and pressure on the uterine region were continued. I used gentle extracting force. When the head had descended into the pelvis, I carried up my finger and hooked it into the child's mouth; but all the force which I thought it prudent to use, was not sufficient to accomplish the delivery, and the mother was utterly unable to assist. Anxious for the safety of the child, I introduced one blade of the forceps, by means of which I was enabled to complete the delivery of the fourth child, within six hours from the birth of the first.

The situation of both mother and child was now very alarming. Whilst I was using the common means for the resuscitation of the child, I directed the assistants to keep a constant pressure on the uterus, to apply a cloth, wet with spirits and water to the vulva, to administer cordials, and apply volatiles to the nose. As soon as the child began to gasp for breath, I gave it to an assistant, to be immersed in a warm bath, while I directed my attention to the mother. I found blood flowing from the uterus in considerable quantities. The placentæ were detached from the uterus, and being connected together, were removed without difficulty.

Notwithstanding every means was used to induce uterine contraction, the removal of the placentæ was followed by a gush of blood. Recollecting the practice of Professor Willoughby in such cases, I wet my hand with spirits, carried it

into the uterus, and gently tapped its walls with my fingers, and although life could scarcely be detected, yet the uterus contracted, and the hemorrhage ceased. A broad bandage was immediately applied about the abdomen, a little wine and water with laudanum administered, and heat applied to the extremities. In about two hours the pulse at the wrist began to rise. I directed the attendants to keep her quiet, with a cloth wet in cold spirits applied to the vulva, and ordering her a little wine and water occasionally, with powders of acetate of lead and opium, to be given if the hemorrhage should return, I left her.

The history of the children is briefly this. They were born on the evening of the 14th of December. Though they were extremely emaciated, they appeared to be doing well until the evening of the 15th, when they all died within the space of two hours. I attributed their loss to the inclemency of the season, and the want of proper provision for their comfort. They were wretchedly clad, and had nothing but a miserable hovel to protect them from one of the severest storms of winter.

The weight and dimensions of the children were as follows:

Sex.	Weight. lb. oz.	Length inches.	From nape of the neck to crown.	From one pariet. protub. to the other.
First, a boy	3 8	17½	3¼	3⅛
Second, a girl	2 4	14¾	3	2¾
Third, a boy	3 4	16½	3⅛	3
Fourth, a girl	2 14	15¼	2⅞	2⅝
Aggregate	11 14			

The mother was attacked with puerperal fever, from which she slowly recovered. She was unable to walk without assistance, until the middle of May following. The menses returned in April, but did not appear a second time until the 12th of June, after an interval of six weeks. About this time she became again pregnant, and, on the 16th of March, 1822, became the mother of two fine healthy girls. On the ninth day

she suffered considerably from uterine hemorrhage, and still remains in a feeble state of health.

Each child had its separate involucrum, the placentæ being united by their membraneous edges forming in this way one united mass, weighing, when washed, two pounds ten ounces. It was deposited in the museum of the Medical College at Fairfield, where it has been preserved for the use of the professor of Obstetrics.

ART. IV. REPORT *on the* YELLOW FEVER *which prevailed in* NEW-YORK *in* 1822. *In a letter to his honour the Mayor of New-York. By* JOSEPH BAYLEY, M.D. Health Officer of the Port of New-York.

New-York, December 9, 1822.

MESSRS. EDITORS,

I enclose. for insertion in your Journal, a Report on the Yellow Fever which prevailed in this city during the last season. This report will shortly be laid before his honour the Mayor.

Yours, respectfully,

JOSEPH BAYLEY.

TO THE HONOURABLE STEPHEN ALLEN,

PRESIDENT OF THE BOARD OF HEALTH.

My time has been so fully employed with quarantine duties, and recently with the removal of my family to the city, that I have not had leisure until now to examine the necessary documents, which would enable me to state some facts, and offer some observations on the important subject contained in your letters, and I am persuaded that you will accept of these excuses for my seeming neglect.

In your high and responsible station of Chief Magistrate of

this city, and of President of the Board of Health, the cause or source of the deplorable calamity with which we have been afflicted, has deservedly claimed your unwearied attention and close investigation.

The first vessel mentioned in your letters as one of the probable sources of the late pestilence, is the ship Shamrock. She arrived at the quarantine on the 11th of June from Havana, via Matanzas, and after the cargo had been discharged, and the vessel purified, she was hauled to the wharf at or near Rector-street, on the 24th of that month. It is my opinion, that this vessel was not the cause of the disease, for the following reasons:

First. Her crew was composed of eleven *eastern seamen*, who are more liable to the diseases of warmer climates than southern sailors, yet they enjoyed uninterrupted health, from the time they sailed from Wiscasset last February, until their arrival in this port, and I have not heard that any of them were sick while they remained here.

Secondly. When this vessel was at Havana in April and the beginning of May, that place was declared to be unusually healthy, the truth of which was confirmed by the fact, that of one hundred and ninety-two persons, the crews and passengers of thirteen vessels from Havana, who arrived here in the month of May, only one person was sick during the voyage, and his disease was diarrhœa.

Thirdly. The Shamrock sailed in ballast from Havana, May 9th, for Matanzas, which place was also reported to be healthy; there she was loaded, and proceeded, June 1st, from thence to this port. An evidence of the healthiness of Matanzas, while the Shamrock was there, appears from the circumstance of one person only being sick out of one hundred and twenty-three, who came here in ten vessels from that port in the months of May and June, and he died of consumption. I was also informed by one of our most respectable merchants, who had resided several months at that place, and arrived here a few days after the Shamrock, that Matanzas had been perfectly healthy for many months, and was so

when he left there. These facts ought to convince us that this ship was not infected.

The schr. Florida is considered by many persons as having been the source whence the deadly malady arose; not that she was a sickly vessel on her late voyage, but because she is supposed to have introduced the yellow fever into St. Augustine the year before, after her return from Havana, at which place, or on her passage from thence to St. Augustine, several of the crew were sick with that disease. Capt. Johnson, her present commander and owner, informed me, on his arrival here a year ago from St. Augustine, that the first persons taken sick with yellow fever at that place, were those who washed the clothing which he used during his illness, and who lived in the house where the clothing was sent. Now admitting the foregoing circumstances to be true, still we must allow, that the frosts which occurred here from the eighth to the twenty-sixth of November, while she was in this port, must have eradicated the remains of any infection which might lurk in this small and shallow vessel. She is less than six feet deep in her hold, and only thirty-three tons burthen. She sailed from New-York November 26, 1821, and arrived here June 21, 1822. While she was away, she was employed exclusively in aiding vessels wrecked on Florida reef, from which place she made several trips to St. Augustine and St. Mary's. She was also once or twice off the harbour of Havana for a day or two last February. Capt. Johnson's only business there, was to receive payment for transporting troops or emigrants from Florida to Havana the preceding year: he did not go into the harbour, as it would subject him to pay port-charges for his vessel. From the time that she sailed from New-York until her return, a period of seven months, not a person was taken sick during her absence, although the crew were changed more than once. This, certainly, is a strong proof that no infection remained in her, or it probably would have shown its effects by occasioning sickness among her crews, as they had been on board several months, while she was in a southern climate. Her cargo, which merely put her in ballast, consisted of a

little old copper and iron, a small quantity of snuff, junk, and fruit. Her hatches were off all the passage. Her bottom was so worm eaten, that she required constant pumping. After the Florida was purified, she was hauled to the wharf near Rector-street, on the twenty-fifth of June.

The schooner Nile has also been mentioned as a suspicious vessel, but as she did not arrive at the wharf near Rector-street until ten days after the fever commenced, it is unnecessary to examine any further into her situation, than to remark, that she was so perfectly clean, that the most scrupulous could not find fault with her. None of the crew were sick during the voyage, and the port she came from was healthy while she remained there.

These are the only vessels supposed to have been infected, which were at the wharves near Rector-street, that I have heard of. If there are any others which have been at that place, whose condition you are desirous of having examined into, I will with pleasure give you all the information in my power.

The local nuisances at or near Rector-street, to which pestilence has been attributed, are very limited in extent, when compared with very many parts of our city, which have been totally exempt from the fatal fever of the present season. Its northwestern exposure renders it some degrees cooler, than it is on the southeastern side of the town. The made ground there is composed of the best materials, and that which was dug up to the depth of three feet a month ago at the foot of Rector-street, to connect the new pier erected there to the bulkhead, was, on close inspection, found to be clean gravel and stone-cutter's chips, and entirely free from any offensive smell, although this is the very place, which many imagine was impregnated with the pestilential materials supposed to be conveyed in the rain water that falls on, and filters through Trinity church-yard, and which being there deposited, have been the cause of the late pestilence.

The peculiar nature of contagious or infectious air has not yet been discovered: its existence in any place is known only

from its effects, and from these alone can we at present fairly draw our inferences.

The facts are numerous and indisputable to prove, that the yellow fever has been communicated to individuals who have visited vessels on board of which one or more persons have had that disease, or which vessels came from a place where it prevailed; and they have imbibed the infection, not only while the cargoes were on board, but after they were discharged, and the vessels thoroughly cleansed. A noted instance of this fact occurred this year on board of the United States brig Enterprize. After the sick had been removed to the hospital, the well were landed, that they might not be any longer exposed to the infected air of that vessel, and to afford an opportunity of purifying her, which was done by slaking lime in her limbers, and white washing her throughout, not omitting her iron ballast; in which operation three casks of lime were used; and she was well ventilated by four windsails constantly in her hatchways. The crew were so intemperate during the five days they remained on shore, that we were under the necessity of sending them again on board. The brig had been purified in the interim. But it was soon evident that she was still an infected vessel, for in six days after the crew were sent on board four men were taken sick with yellow fever, and in the course of five days seven more had the same disease, making a fourth of all the men on board; and five of the eleven taken sick died. These persons must have been infected after their return to the brig, and subsequent to her purification, otherwise the disease would have been excited in them as it was in the case of some of their shipmates, from their irregular living, and exposure to the weather, by lying on the ground at night. Those men must have been more predisposed to receive infection after their return to the brig, than they were previously to their landing: in consequence of having lived in a pure atmosphere, and having been debilitated by repeated intemperance.

This fatal evidence of the cause of the disease still lurking in the Enterprize, induced us to have the crew brought on shore

again, and recommence the purification, which was done by using two more casks of lime, and letting into her hold daily several feet of water, and keeping up windsails. Six men were left to take charge of her, who were directed not to sleep below deck, and whose duty was to pump out the water and trim the windsails. On the second of August, twenty-five days after her arrival, and after repeated white-washing, letting in water, and constant ventilation, one of the sailors obtained permission of a lieutenant to take his wife on board; this woman was taken sick with yellow fever on the ninth, and died in the Marine Hospital on the eighteenth of that month. In the case of this vessel we have a positive proof that this disease comes from abroad, and that its infection continues to act after an abundant use of lime, being ten times the quantity employed in white washing a house of the same dimensions, besides the immense quantity of water let in and pumped out, and a constant supply of fresh air conveyed into her hold by windsails. Here no animal or vegetable matters were to be seen, or otherwise detected, whose decomposition could keep up this poisonous air, especially after she was first cleansed.

It is asserted by many persons that animal and vegetable decomposition is the cause of the yellow fever in the West Indies; and as our climate is equally as warm in summer as it is in those islands, the same effects must follow like causes here as well as there, consequently the yellow fever may also be generated in our city. This reasoning is philosophical, and if the premises are well founded, we should not hesitate to submit to the conclusions drawn from them; but for the sake of argument, I would ask if there is no difference between the duration of heat there and here? Does not this circumstance make a very material difference in the productions of the two countries: and why may it not make as great a difference in the products evolved in their decomposition?

Wherein the constituent parts of an infected atmosphere differ from a pure one is unknown. Until this mystery is developed, and the gases which arise from the decomposition of animal and vegetable substances here, are proved to be pre-

cisely similar to those which are supposed to generate the yellow fever in the West Indies, we cannot, for the reasons assigned above, consider them to be the same.

This theory of the disease is not supported by the facts furnished even in our own city. If the reasons upon which it is founded are correct, the yellow fever should occur, at the same time, in all those places of our city where nuisances exist, in the interior, as well as along our wharves; and especially at all the points where it had before commenced, as the same materials which had once generated it, could not fail to reproduce it; and where receptacles of filth most abound, there the infection should be extended to a greater distance, and the virulence of the disease greatly increased. Seldom a year would pass without our being smitten with pestilence; and its annual return would be usually at or near the same time of the year, only a little varied by the previous state of the weather.

It is well known to our inhabitants, that as often as we have been visited by that awful calamity, it has never commenced at the same place, and never has it began in the interior of our city. It has commenced its ravages as early as June, and as late as September, in seasons nowise remarkable for their peculiarity; while we have some years escaped the pestilence, when the combined action of heat and moisture has been excessive; which causes are universally admitted to give increased activity to the decomposition of putrescent substances. In the present year we have witnessed its spreading over the highest, cleanest, and most airy part of our city, and far from the spot where it commenced.

As it does not appear that the vessels above referred to, or the local causes that existed in Rector-street, afford a satisfactory explanation of the origin of the late pestilence, I beg leave to call your attention to another foreign source, that has hitherto received too little attention, but which, on close investigation, will probably appear more adequate to its production than either of the foregoing. It has been so repeatedly proved that vessels from sickly ports have given rise to the yellow fever, where no local causes existed for its production, that

even those who believe in its domestic origin admit that it may be imported. If, then, pestilential air escapes from a vessel at the wharf, while the cargo is discharging, may not the cargo of such vessel, immediately transported in lighters, under particular circumstances, retain a portion of pestilential air, and the cause of disease be conveyed to the city in this manner? The following statement of the transportation of cargoes from sickly Havana vessels, or which sailed from that port when yellow fever prevailed there, has been carefully compiled from the records kept by the revenue officer at quarantine.

June	14	3	lighter loads from the brig Rapid,	363	boxes of sugar.
	15	2	do. do.	189	do.
	23	1	do. brig Spanish Soldier,	77	do.
	29	1	do. do.	100	do.
July	1	2	do. brigs Span. Soldier & Abeona	177	do.
	2	3	do. brigs S. Soldier, Abeona, and Ambuscade,	260	do.
	3	3	do. do. do. do.	181	do.
	5	4	do. do. do. do.	216	do.
	6	5	do. do. do. do.	384	do.
	8	4	do. do. do. do. and ship Eliza Jane,	317	do.
	9	1	do. do.	106	do.
Aug.	5	1	do. brig Packet,	70	do.
	6	1	do. do.	70	do.
	7	2	do. do.	145	do.
	8	1	do. do.	75	do.
		34		2730	

From the above particulars it appears that 2730 boxes of sugar were transported in thirty-four lighters from several sickly Havana vessels, (and those not sickly were navigated by persons who had made frequent voyages to that port,) between the fourteenth of June and eighth of August, and landed at the wharves, within the limits of one hundred and twenty yards on each side of Rector-street: but the circumstance to which our attention ought chiefly to be directed, is the important fact, that more than two-thirds of the whole quantity, amounting to 1918 boxes, the entire cargoes of the three brigs

Spanish Soldier, Abeona, and Ambuscade, and part of the cargo of the ship Eliza Jane, were conveyed in twenty-four lighters, between the twenty-eighth of June and ninth of July; and even nineteen of the twenty-four in the short period of six days, from the second to the eighth of August. The lighters were generally loaded before noon, at which time the weather was frequently calm, consequently the infected air adhering to those boxes could not have been completely driven off in passing them from the hold of the vessel to the hold of the lighter. The heat of the weather in the shade at Staten Island, between the twenty-eighth of June and ninth of July, was above eighty degrees at two P. M., and upwards of seventy-seven at eight A. M. and six P. M.; and for the whole period between eight A. M. and six P. M. the average was more than seventy-eight degrees. It has been erroneously stated in some of the public prints that it was my opinion that the pestilence had been conveyed in boxes of sugar. I never entertained such an idea; but I conceived it possible that infected air, shut up in the hold of a vessel during a West India passage, would as readily pervade, and be retained in the spongy texture of rough pine boards, of which those boxes are made, as the more dense structure of smooth oak planks, of which vessels are built; and that several lighters, loaded with such cargoes, being landed daily at or near one place, would probably convey as much pestilential air as a single vessel discharging at the wharf. and which has been known to produce the disease. In the instance before us we have the cargoes of three vessels discharging at the same time, and near the same place.

It may be asked, if the cargoes from sickly vessels, or from sickly ports, have not been brought to other parts of our city where no evil has been produced from them; and if the pestilence can be conveyed in that manner, how could such places be exempt from it?* To answer this question, I have ascer-

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* The cargoes of the following vessels from Havana were discharged at other parts of the city than at or near Rector-street:

tained the amount of produce from Havana, which has been landed at all parts of the city, other than was discharged near Rector-street, and find by the revenue officer's reports, that 1518 boxes of sugar, and 361 hogsheads and 107 barrels of molasses and sugar, have been transported in twenty-seven

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At Old-Slip.

June 20	1	lighter load	from schooner Retrieve,	60 boxes of sugar,	50 bags of Pimento.
22	2	do.	from schoon. Retrieve,	172 do.	20 hhds. do. and 50 bags of Coffee.
28	1	do.	from schoon. Cerena,	30 do.	26 hhds. do.
29	1	do.	do.	35	do.
July 1	1	do.	do.	35	do.

At Fulton-street Wharf.

June 21, 22, 24, 25	1	do.	from the brig Venus	256 hhds. Molasses.

At Stephen's Wharf.

June 27	2	do.	from the schr. Virginia Packet	135	boxes of sugar.
28	2	do.	do. do.	191	do.
29	1	do.	do. do.	82	do.

Between Old and Coffee-House Slips.

July 12, 13, 16	1	do. each day,	sch. Kennebeck Trader,	200 do.	115 hhds. and barrels do.

Near Old Slip.

18	1	do.	brig Fame,	103	do.
19	1	do.	do.	47	do.
20	2	do.	do.	140	do.
22	1	do.	do.	106	do.
23	2	do.	do.	141	do.
Aug. 1	1	do.	brig Packet,	84	do.
5	1	do.	do.	67	do.
	27			1565 boxes—497 hhds.	

The crews of those vessels, except the brig Venus, were healthy during the voyage: that brig lost one man, June 7. Dr. Morrel, a passenger on board, it is said, died of bilious, and not yellow fever.

lighters, between the twentieth of June and the eighth of August; (after which time no cargo was brought here from sickly vessels, except such as was purified by washing the casks, &c.)

But the greatest number of lighter loads, landed at or near one place in six successive days, did not exceed seven, which occurred only in a single instance. From the eighteenth to the twenty-third of July these lighters conveyed the cargo of the brig Fame, on board of which vessel no person had been sick during the voyage; whereas nineteen lighter loads, from three vessels, were discharged in the same number of days, near Rector-street, from the second to the eighth of July. This, in my opinion, is an obvious difference; for a small quantity of pestilential air may be inoperative by mixing with a large portion of atmospheric air, while a greater quantity, existing in a more concentrated state, proves fatal. The fever which took place at the quarantine ground in 1821, when many vessels were there driven on shore, is an example directly in point: it was evident that the foul air issuing from those vessels at the same time contaminated the pure atmosphere of the country, which occurrence never before happened from a small number at the wharf. Other circumstances, that might make a difference between the cargoes which were landed at Rector-street and other places, could have been ascertained, when the vessels arrived here, if I had been aware that any such particulars would have been of any practical use: such as the place where the vessel laid in the harbour, as some positions being healthier than others; and whether the cargo came immediately from the country, or had been stored in a sickly part of the city. It is a well authenticated fact, that vessels coming from Havana, loaded with molasses, are healthier than those loaded with dry sugar, although the bilge water of the former is much more offensive than that of the latter: the reason of this difference in the healthiness of their crews is attributed to the circumstance that the first are loaded at a village called the Regulars, situated on the opposite side of the bay to the city of Havana, where it is generally healthy.

My object in this investigation is not to support any theory on the subject, but to endeavour to come at the truth, and learn if any reason could be assigned that would account for the difference, which appears to have been produced by the cargoes landed at Rector-street and other places, that measures may be adopted to guard us in future, if the disease has been introduced in this manner. Although the law requires the health officer's permit before the cargo of a sickly vessel can be brought to the city, yet it has been the practice heretofore to detain at quarantine only those articles which were damaged or generally supposed to be susceptible of retaining infection.

The commerce of this port with sickly West India ones is so extensive, that all the cargoes brought here from them, cannot be stored at quarantine without providing additional buildings; and the merchants could not examine them there without being exposed to the foul air of damaged and infected articles, as well as to sickly vessels at the wharf; this would produce considerable embarrassment in their business, by impeding or preventing the sale of their property. It would therefore be proper to discriminate between articles which come from ports where the yellow fever prevails, as they are more or less liable to retain infection, that those least liable might be stored at an intermediate place, between the city and Staten Island, to be ventilated for twenty or thirty days before they are brought to the city, and where the merchants could have free access to them with little hazard. West India produce, coming coastwise, which has not been landed in the United States the same length of time, and which is not susceptible of purification, except by ventilation, ought also to be taken to the same place.

I conceived it to be my duty to offer to you my views on this particular point, although it may appear trivial to some; yet on this highly important and interesting subject, which is wrapt up in so much mystery, and in which the lives and property of our fellow-citizens are involved, every circumstance which in the smallest degree can throw any light upon it, should

be unreservedly communicated, especially if it can be of any practical use.

During the late pestilence, eighty-five persons were sent to the Marine Hospital from the city and fort Richmond, forty of whom died. Sixty-five had yellow fever, and of this number thirty-seven died, eighteen of whom had black vomit; from which it appears that the recoveries were in the same ratio as in the city. This fact ought to remove the erroneous opinion entertained by the public, that the sick sustained much injury in being sent to Staten Island. It is readily admitted, that sick persons, surrounded by their relatives and friends, and supplied with every comfort, might be injured by their removal to any public institution, particularly to a Lazaretto, in consequence of the shock given to their feelings. Such, however, was not the happy condition of the persons sent to the Marine Hospital. They were generally destitute, without attendants; and living in the infected district, their removal to a healthy place, where they could obtain what their situation so much required, must have contributed to their comfort, and in many instances to their recovery.

The characteristic symptoms of the disease in those persons who were sent from the upper and lower infected districts, and those from the United States' brig Enterprise, were the same, with similar variations in acuteness, as are observable in other febrile diseases. The first or inflammatory stage in those from the Enterprise was generally more violent and longer protracted, than in those who were sent from the city; which probably was occasioned by the former being more robust and plethoric than the latter, nearly half of whom were females, but we had not a frequent opportunity to judge how violent their fever was at its commencement, as most of them had passed the first stage before they were removed.

We did not observe that the malignancy of the disease was in any degree lessened in those persons who contracted it at a distance from the place where it began, nor that its violence was in any measure mitigated in those who were infected a short time before the pestilence terminated.

The more I see of yellow fever, the stronger is my conviction, that it is a different disease from bilious remittent fever. As they are both diseases which prevail in warm climates, and often exist in the same place, it is supposed that they have a common origin, and this no doubt is the reason why the great majority of physicians view them as the same disease. This was formerly my opinion, but which has been long changed, by observing the symptoms and termination of both diseases at the Lazaretto. The one is brought from the West Indies, the other is usually contracted in our southern ports, where extensive marsh exhalations abound, and it is unquestionably produced from that cause: this opinion is confirmed by its frequently terminating in an intermittent, which is the common disease of low and marshy countries. But of the fifty recoveries from yellow fever this year at the Lazaretto, not one of them had intermittent fever. To ascertain the relative proportion of deaths to recoveries between those two diseases at the Marine Hospital, I have examined the register of that institution since it was opened at Staten Island in the year 1799, to the present period, and find recorded in it 508 cases of yellow fever, 283 of which terminated fatally, making the deaths as 4 to 7, and 308 cases of bilious remittent fever, of which 86 died, making the deaths only 2 in 7. This very great disproportion in the fatal termination of the two diseases is in itself a very obvious distinction; moreover I have witnessed the greater part of those 86 deaths, none of whom had the coffee-ground black vomit.

Many persons who believe in the local origin of the yellow fever in our city, attribute it to the gas arising from the decomposition of animal and vegetable matters generated at some depth below the surface of the earth. It was readily perceived by those persons, that if they ascribed it to the putrefactive process, which takes place on the surface, then its effects would be felt much earlier in the season, than at the last of August or first of September, at which period the pestilence has often begun; for the heat of the weather was as great and often greater during the two preceding months, than it was when the disease commen-

ced; consequently the noxious gases produced from such decomposition, must have been diffused in the atmosphere a much longer period, before its evil consequences are experienced, than facts will warrant us in allowing. But to account for the continuance of the disease in autumn from this cause, was a difficulty of still greater magnitude, as the evolution of gases from the decomposition of putrescent substances on the surface would be checked or prevented by cool weather.

Since then the gases from the decomposition of animal and vegetable substances on the surface of the ground, do not satisfactorily explain the origin and continuance of yellow fever, we were induced to examine into the more plausible theory, that the noxious gases which are said to give rise to it, were produced at some depth below the surface of the ground, which would better explain why the disease did not appear until the sun had been long acting on the earth and imparted its heat to it, and why it continued after cool nights had checked decomposition on the surface. For this purpose we made a number of experiments between the 18th of September and 26th of October, by burying the thermometer in the earth from three inches to seven feet, and letting it remain there from six to 12 hours. In thirty experiments made on different days at or near sunrise, the thermometer lying on the ground, stood at some point between 35 and 72 degrees, and when taken out of the ground after being buried twelve hours at various depths, from three inches to seven feet, it ranged between 44 and 67 degrees. To the depth of nine inches, the temperature of the earth was altered by alternate changes of atmospheric heat, but very little variation was observed in the thermometer when buried between one foot and seven feet, although the change was considerable on the surface of the ground. For instance, when the thermometer, lying on the ground, was at 52 degrees on the 24th, and 64 degrees on the 27th of September, it was 64 degrees one foot below the surface on both those days. Also, on the 5th, 9th, and 11th of October, when the instrument was laid on the ground, it was 51, 54, and 69 degrees, but when it was dug up from the depth

of four feet the same days and at the same time, it was 64 degrees each day.

In twenty-two experiments made in the same manner, between one and three o'clock, P. M. the thermometer laid on the ground in the sun ranged from 76 to 91 degrees, but when buried for six hours in the earth at various depths, as between three and 12 inches, it varied from 56 to 76 degrees, according as it was placed near to or at a distance from the surface; for instance, on the 18th, 21st, and 23d of September, the thermometer laid on the surface of the ground, in the sun, was 84 90, and 90 degrees at one o'clock, P. M. but after being buried six hours, and the sun acting all the time on the same spot, when the instruments were taken up at one o'clock on the same days, from the depth of three inches, stood at 74, and at the depth of six inches at 70. But when placed below the depth of one foot, it was as little affected by the heat of the sun as when buried in the shade, for it stood at 68 and 64 degrees, at various depths between one foot and seven feet, while the instrument laid on the same ground in the sun varied between 80 and 91 degrees. From those experiments made in ground not materially different from that which I saw dug up at the foot of Rector-street, it appears that the earth a foot below the surface, in the latter part of September and in October, varies very little in its temperature when the sun-beams have been acting upon it for seven hours, or when it is shaded by twelve hours night, the heat being about 64 degrees.* This is a much lower temperature than I have ever heard would give origin to the cause of yellow fever; the heat of the earth one foot below the surface not being more than 64 degreee in Sep-

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* On the 24th and 25th of September two thermometers, which had been buried for twelve hours, to the depth of nine, twelve, fifteen, and eighteen inches, when taken up at sunrise, stood at 63 and 64 degrees, although the instrument lying on the surface was 52 and 50 degrees. But when the thermometers were buried at similar depths, for seven hours on the same days, the sun acting on the same ground all the time, they stood at 63 and 64 degrees at 2 P. M. although the instrument laid in the sun on the same place was 80 and 85 degrees.

tember and October, we must therefore infer, that the continuance of the disease in autumn by the extraction of gases from the depth of several feet in the earth, is not a more satisfactory explanation of the cause of it, than that of the noxious gases emitted from the surface of the ground. We are not prepared to adopt this theory of the origin of yellow fever, unless adequate fermenting masses, producing internal heat and sending off pestiferous gases, are proved to exist constantly in the neighbourhood where pestilence prevails.

I have thus presented to you a plain statement of facts, upon which I have formed my opinion of the introduction of the cause of the late pestilence, and offered a few observations connected with the subject, which I have been induced to do from a persuasion, that they may be of some practical utility, and contribute to the improvement of our quarantine system.

With great respect,

Your most obedient

and very humble servant,

JOSEPH BAYLEY.

ART. V. CASE *of* MENTAL IMBECILITY, *arising from a contusion of the Anterior Tibial Nerve. By* JAMES ANDERSON, M. D. of New-York.

THE case which I now communicate for publication in the New-York Medical and Physical Journal, occurred in the practice of the late Dr. Richard S. Kissam, while I was one of his pupils, in the year 1818. As the eldest student, I was frequently requested by the doctor to give attendance on his surgical patients, and the case, the particulars of which I now send, was observed by me throughout its whole progress. I am inclined to think it sufficiently interesting, because it furnishes some striking facts concerning the connexion between the nervous system and the intellectual powers.

George ———, the son of G. T. ———, aged about fourteen years, a native of this city, of a plethoric habit of body,

and of the nervous temperament, received an injury on the top of the foot, from a stone thrown with violence by one of his playmates. From the structure of the part upon which the momentum of the stone was expended, it contused that portion of the anterior tibial nerve, that lies between the metatarsal bone of the great and adjoining toe. The first effects of this injury were trivial, causing only a slight degree of pain and uneasiness upon motion, and were partially allayed, for six or seven weeks, by the ordinary domestic applications of tepid vinegar and water, liniments, &c. About eight or ten weeks after the accident, the pain increased, accompanied with a slight swelling of the part, and also with an extension of pain towards the ankle and leg. At this time Dr. Kissam was sent for, who concluded that the existing pain was induced by an inflammation of the soft parts where the blow was inflicted, and unconnected with the nerve itself. His prescriptions were, accordingly, such as were indicated to lessen the sufferings of the foot: such as quietness, saturnine applications, and fomentations. From this treatment no benefit was manifested; the disease continued to increase in violence, and now extended itself further up the trunk of the nerve, and affected the adjacent muscles with spasms, which at times were mitigated in violence, but generally continued with about the same force. The muscles especially affected in this way were those directly supplied by branches from the anterior tibial nerve. The constitution was involved in this irritation; the pulse increased in frequency and force, though the digestive organs performed their duty with wonted regularity, if not increased strength. The pain increasing in violence and extent, blisters were now made use of, and constantly renewed. Saline cathartics and other purgatives were occasionally administered, and large anodynes prescribed with a view to obtain rest.

Having fairly tested the effects of the above remedies, and finding them ineffectual, closer examination evinced that the nerve I have mentioned was primarily the seat of irritation. It was now determined in consultation to have recourse to the most powerful antispasmodics and nervines. The pain had

extended above the knee, and was almost insupportable. Assafœtida, cicuta, and belladona, produced no sensible relief; the symptoms became more formidable; the spasmodic action of the muscles was most conspicuous in the extensor longus pollicis pedis, and drew the great toe perpendicularly to the surface of the foot; the least effort to return it to its natural situation, or even the slightest touch would excite the most anxious sufferings: these lasted from three to five minutes.

At about three months from the time of the accident the whole system became involved in these spasmodic irritations, and his nervous energy enfeebled. The common sensorium was disturbed: he lost his reasoning and recollection; was unable to distinguish occasional visiters, or recognise even his parents, or any of the members of the family; his mind became imbecile and idiotic; he was deprived of the ability to read, or distinguish the letters of the alphabet. As the pain extended up and beyond the thigh, it spasmodically affected the action of the muscles of respiration; and at the invasion of each paroxysm of suffering his breathing became more frequent and laboured. This frequency of respiration was generally the first indication of approaching exacerbation. Though his distress was most acute, yet he gave no utterance to his feelings. While the paroxysms were on him he would roll his fist, and imitate the actions of a pugilist, but with much greater violence and rapidity, often striking his nearest and best friends, and all around him. If no person or thing was within the reach of his arms, the force of his actions would be lost in the air.

From the evidence of the local derangement of the tibial nerve thus involving the whole system, and the inefficacious results of all medicinal remedies, it was determined to have recourse to other means, and a surgical operation was agreed upon. A division of the anterior tibial nerve was deemed adviseable. This received the approbation of his parents.

Accordingly, the anterior tibial nerve was divided above the ankle, from a belief that it would more effectually cut off the connexion between the injured part and the nervous sys-

ters at large. The operation was performed in the presence of Dr. William Barrow, the consulting physician and surgeon, and myself. An incision was made about two inches in length just on the outside of the tibia, beginning about four inches above a transverse line drawn from one maleolar process to the other: the second incision was a corresponding division of the facia of the leg: the third separated the extensor longus pollicis pedis from the tibialis anticus, down to the anterior tibial vessels and nerves. The blood vessels being detached from the nerve, the nerve was divided by a bistoury, and about an inch of it cut out. Immediately upon this separation of the nerve, the extensor muscle of the great toe became so relaxed as to admit the toe to resume its wonted situation by the side of its fellow. There was an almost immediate alleviation of all those extraordinary morbid phenomena, that had exhibited themselves in the unfortunate sufferer. The local affections became less distressing, and in a short time wholly disappeared. The parts could be handled with impunity, and no excitement arise thence. There was a partial numbness of the top of the foot after the division, but the natural feeling was shortly after restored. The wound was dressed in the ordinary manner, by adhesive straps, and healed in the course of ten or twelve days. He has since enjoyed good health.

New-York, December 1, 1822.

ART. VI. AN *Examination of the Medico-legal Question, whether, in cases of* INFANTICIDE, *the* FLOATING OF THE LUNGS IN WATER *can be depended on as a certain test of the child's having been born alive.* By JOHN B. BECK, M. D.

IN the whole range of forensic medicine, there is not a question more important, and at the same time more difficult, than the one which it is proposed to discuss. It has divided the opinions of medical jurists from the earliest periods, and even at the present day it still remains the subject of controversy.

When it is recollected, how great and just an importance has been attached to it in trials for child-murder, and how embarrassing to courts and to juries have been the contradictory sentiments advanced concerning it by medical witnesses, the propriety of the present investigation cannot be questioned.

For the purpose of rendering the subject as distinct as possible, I shall first state the test, and then consider the different objections which have been brought against its accuracy.

If the lungs of a child which has never breathed be put into water, it is found that they are specifically heavier than the water, and of course sink. On the contrary, if respiration has once taken place, the lungs being specifically lighter than water, will then float. From these facts the general conclusion necessarily follows, that when the lungs of a child float in water, it must have respired, and therefore must have been born alive. And on the other hand, when they are found to sink, it is an evidence that the child has not breathed, and therefore was not born alive.

By the French this is denominated the *hydrostatic test* of the lungs, a name both convenient and appropriate, and I shall therefore adopt it in the following discussion.

Let us now see whether it is safe to trust to the evidence furnished by this test, by considering the different objections which have been urged against it.

I. It has been objected, that a child may have been born dead, and yet the lungs will float in water, from having undergone putrefaction, and therefore, it is argued, that the mere floating of the lungs is no decisive proof of previous life.

With the view of giving this objection its full force, it may be proper first to consider the effects of putrefaction.

Strange as it may appear, it has nevertheless been a subject much debated, what the effects of putrefaction are upon lungs that have never respired; some asserting, that it renders them specifically heavier than water, and consequently, that they will sink when thrown into that fluid; while others, of equal respectability, maintain a contrary opinion. Both parties adduce experiments in proof of their particular doctrines. The

only solution that can be given to these contradictory results, is by admitting that all the experiments have not been performed with sufficient care, so as to lead to conclusions uniformly just. Every thing depends upon the *manner* in which they are conducted. The most accurate, I believe, were those performed by Mayer, and as they place this subject in a very just point of view, and relieve it of much of the obscurity in which it has been involved, it may not be improper to present a summary of his observations. From a very extended series of experiments, continued during a number of years, and executed with the utmost care and precision, Mayer found, on putting into water the lungs of still-born children, in whom, of course, no sign of respiration or life had appeared, that they sunk to the bottom. After an interval of two or three days, the water in which they were left became turbid; the lungs changed in colour, and increased in volume; here and there an air bubble arose to the surface of the water, and at the same time a putrid odour became perceptible. All these appearances continued to increase daily, until the sixth, seventh, or, at the latest, the eighth day, when the lungs, both entire and cut into pieces, floated in the water in which they became putrid. On transferring the lungs to vessels containing clean water, they still continued to float, although on the slightest compression they instantly sunk.

Lungs placed in water, and exposed to the rays of the sun, swam on the sixth day. If they were suffered to putrefy where there was a free current of air, they rarely floated before the tenth or eleventh day. After the lungs had once floated, they remained in that state, emitting daily a more offensive odour, and acquiring an increased volume, until the twenty-first, or at the latest the thirty-fifth day. After that period they gradually sunk down, without a single exception, to the bottom of the vessel, nor did they afterwards betray any disposition to float, although kept for seven weeks, and in some instances a much greater length of time.*

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* Collectio Opusculorum Selectorum ad Medicinam Forensem spectantium, Curante Dr. J. C. T. Schlegel, Leipsic, 1785–1791, vol. i. p. 262. 3, 4.

The foregoing experiments were made in the month of August. The lungs, both entire and cut into sections, were immersed in pure fountain water, and contained in vessels convenient and capacious. In short, every precaution seems to have been scrupulously observed, to render the experiments accurate and satisfactory.

My own experiments on this subject, although not numerous, go to confirm, in every essential point, those which have just been detailed I shall content myself with recording the history of a single one.

Twenty-four hours after delivery, I was favoured by a medical friend with the thoracic viscera of an infant, which had been still-born, after a very difficult and protracted labour. On putting them into a vessel of water, the heart, lungs, thymus gland, &c. were found to sink readily to the bottom, both conjointly and when separated. No symptom of putrefaction was discernible.

August 20, 9 o'clock A. M. This being the third day after delivery, I separated the two lobes of the lungs, and placed them in a vessel of pure rain-water, exposed to the rays of an intense sun: both lobes being at the bottom of the water.

2 P. M. Both lobes floating. Here and there an air bubble visible on their surface. Odour very offensive.

7 P. M. Left lobe sunk; right still floating.

August 21, 9 A. M. Both lobes sunk.

2 P. M. Both floating.

7 P. M. Both floating.

August 22. Both floating all day.

August 23, 9 A. M. Both floating; covered with bubbles of air. On taking out one of the lobes, with a view of cutting off a portion of it, I found it so far dissolved as to render it impracticable. On returning it into the water, it immediately sunk.

2 P. M. The lung which sunk this morning again floating. Both now float, and are horribly offensive.

August 24, 9 A. M. The lung which was taken out and cut yesterday, sunk again: the other still floating.

August 30. No change has taken place in the situation of the lungs since the 24th. Finding the water nearly evaporated, I filled the vessel very cautiously with fresh rain-water, upon which the floating lung instantly sunk.

August 31. Both lungs still sunk, and never afterwards showed any disposition to float, although suffered to stand for better than five weeks.

From the foregoing experiments it thus appears, that in the *incipient* stage of putrefaction, lungs that have never respired will float in water; whereas they will sink, if it has continued long enough to completely destroy their organization, and thus extricate all the air contained in them. These results have been corroborated by numerous other observations and experiments, and their truth cannot be doubted. It seems singular, indeed, that they should ever have been questioned, when a case perfectly analogous is witnessed in every person that is drowned. The body at first sinks; afterwards rises to the surface, when putrefaction has generated air sufficient to render it specifically lighter than water; and finally descends again, upon the extrication of that air.

Such being the effect of putrefaction, it becomes a question of great importance, to determine in what way we may discriminate between the floating of the lungs, as caused by natural respiration, and that which is the result of decomposition.

Independently of the changes produced in the colour and general appearance of the lungs by putrefaction, there are other very characteristic marks by which they may be distinguished.

1. By the appearance of air bubbles on the surface of the lungs.

On this subject, Dr. William Hunter lays down the following rule: "If the air which is in the lungs be that of respiration, the air bubbles will hardly be visible to the naked eye; but if the air bubbles be large, or if they run in lines along the fissures between the component *lobuli* of the lungs, the air is

certainly emphysematous, and not air which had been taken in by breathing."* Jaeger had before this made a similar observation. In lungs floating from putrefaction, he describes the air as contained in the form of bubbles under the external membrane of those organs, where the air introduced by respiration never finds its way.† This rule appears to be founded in truth, and accordingly has been adopted by the best writers on forensic medicine.

2. By the ease with which the air can be extricated from lungs which float in consequence of putrefaction. The evidence of this is to be found in the fact, that if lungs of this description, or any portions of them, be squeezed in the hand, they will immediately sink in water. On the contrary, no compression, however strong, can force out so completely the air from lungs that have respired, as to cause them to sink. This test is insisted upon by Marc, a very distinguished writer on this subject, as the most certain means of discriminating between the effects of putrefaction and respiration.‡

3. By an examination of the other viscera of the body. Numerous observations have established the fact, that with the exception of the bones, the lungs resist putrefaction longer than any other part of the body. Faissole and Champeau, in experiments which they made upon drowned animals, observed that the lungs remained sound, after the whole of the body had become putrefied.◊ Mahon noticed the same fact in his dissections of dead bodies.‖ Camper ascertained, by direct experiments, that the head became so far decomposed by putrefaction, that the slightest force was sufficient to detach the

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* On the uncertainty of the signs of murder, in the case of bastard children. By William Hunter, M. D. F. R. S. Medical Observations and Inquiries of London, vol. vi. p. 284.

† Schlegel's Collectio Opusculorum, &c. vol. v. p. 111.

‡ Dictionaire des Sciences Medicales. Tom. x. art. Docimasie Pulmonaire.

◊ Med. Legal. &c. de P. A. O. Mahon. Tom. ii. p. 400.

‖ Ibid, v. 2. p. 400.

bones of it from each other, as well as those of the arms and legs, before the lungs began to participate in the putrefaction.* From these facts the conclusion evidently follows, that if the rest of the body of the child which is the subject of examination, be unaffected by putrefaction, it may very confidently be inferred, that the floating of the lungs is not owing to putrefaction.

By a careful application of the foregoing tests, little or no difficulty can arise in deciding whether the lungs float from putrefaction or from respiration.

But suppose the lungs are found to be actually in a state of putrefaction, is the physician then justified in drawing any conclusions, or in giving any opinion? Mahon advises in such cases, that it is better for the medical witness to be silent, and to leave to the magistrates the task of finding out other grounds of accusation.† Marc, however, answers this question in the affirmative, and proposes two characteristics to enable him to offer a positive decision. The *first* is, that lungs which have respired, notwithstanding they may have been attacked by putrefaction, always have a crepitus when cut into; whereas those which have never respired, although they float in water, are destitute of this peculiarity. The *second*, and which he considers the most decisive and certain, is this: that upon squeezing out from sections of the lungs the matter developed by putrefaction, they will sink if they are from a child born dead; but, on the contrary, if they are from a child born alive, they will, notwithstanding this, continue to float.‡

II. It is objected, that a child may have been born dead, and yet its lungs may float in water, in consequence of their having been artificially inflated, and therefore, it is argued, that the mere floating of the lungs is no proof of previous life.

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* Dissertation on Infanticide, by W. Hutchinson, M. D. London, 1820. p. 47.

† Med. Legal. tom. ii. p. 400.

‡ Manuel D'Autopsie Cadaverique, Med. Leg.

It has been doubted by some whether artificial inflation of the lungs could ever be effected. Heister states, that he proved by actual experiments, that air cannot be blown into the lungs so as to cause them to float.* Hebenstreit also doubts whether it can be accomplished, in consequence of the mucus which is usually found to fill the fauces of a new-born child.† Roederer, from the failure of his experiments on this subject, was led to the conclusion, that it can only be effected after the child has previously breathed. "A spiritu ori," says he, "inflato pulmones infantis non inflari dilatarique; nisi fœtus aliunde respiraverit."‡ Brendel is still more positive on this point. He believes artificial inflation to be utterly impossible, and assigns two reasons for his scepticism. The first is the resistance which is made by the thorax and diaphragm, and the second is, the difficulty of introducing a pipe into the glottis, without which he thinks it is impossible to inflate the lungs. He adds, moreover, in confirmation of his opinion, that he made experiments upon pups that were killed while yet in the uterus, and although he attempted to force in the air by a bellows, yet no change was effected upon the lungs, and they sunk when put into water.§

A contrary doctrine is however maintained by a very large majority of the most respectable authorities in forensic medicine. Low admits the possibility of it, and tells us that Bohn, together with the medical faculty of Leipsic, concurred in the same opinion.‖ Ludwig says, it is certain that air may be artificially blown into lungs which have never respired, and that they will afterwards float in water.¶ In several experi-

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* Morgagni's Works. vol. i. p. 536.

† D. J. Ern. Hebenstreit Anthropologia Forensis, &c. p. 405. Leipsic, 1751.

‡ Schlegel's Collectio Opusculorum, &c. vol. v. p. 112.

§ Prælectiones Academicæ, J. G. Brendelii in H. F. Teichmeyeri Institutiones Medicinæ Legalis vel Forensis, &c. p. 186. Hanover, 1787.

‖ Johannis F. Low, Theatrum Medico Juridicum, &c. cap. xii. p. 623. Norimbergæ, 1725.

¶ D. C. G. Ludwig, Institutiones Medicinæ Forensis Prælectionibus academicis accommodatæ, p. 97. Leipsic, 1765.

ments made to test this matter by the celebrated Camper, the result was uniformly in favour of this opinion.* Jaeger, Buttner, and Schmitt, concur in the same, as do most of the French and English writers.

From the foregoing detail of authorities it is quite evident, that although artificial inflation of the lungs of a child born dead, is a thing perfectly practicable, yet it is not accomplished with as much facility as many have imagined. This consideration I conceive to be important, because it tends to weaken very much the force of this objection to the hydrostatic test. Still, however, the objection holds good, and there are not wanting occasions when artificial inflation might be attempted. It is not incredible that it might be the result of malice, designed to injure the innocent mother; or of maternal tenderness endeavouring to resuscitate a lifeless child. It becomes, then, a matter of great moment, to determine whether the existence of air in the lungs be the product of nature or of art: and it is fortunate for the cause of justice, as well as humanity, that this can be done.

The first test which I shall notice for this purpose, was originally proposed, I believe, by Buttner, and is founded upon the difference of the fœtal and adult circulation of the blood. In the former, it is well known that the blood does not pass through the lungs; whereas, as soon as respiration commences, the old passages are closed, and the whole mass is forced through those organs. If, therefore, a child has been born dead, the arteries and veins of the lungs are found destitute of blood, and in a collapsed state, notwithstanding any artificial inflation that may have been practised upon them. On the contrary, the vascular distention of the pulmonary organs, proves that the child has breathed; for nothing but natural respiration can produce this effect.

A second method of determining this question is, by taking the weight of the lungs, and comparing it with the weight of the whole body, according to the celebrated test of M. Plouc-

* Schlegel's Collectio Opusculorum, &c. vol. v. p. 112.

quet. As this is a test of great value, it will be necessary briefly to explain the principle upon which it is founded.

From the peculiarity of the vascular system in the fœtus, only a very small portion of the blood goes the round of the pulmonary circulation. As soon as respiration commences, a change is effected, and the whole mass of the blood passes through the lungs, in order to undergo the necessary process of oxygenation. From this it appears obvious, that the fœtal lungs must be considerably inferior in weight to the same organs after respiration has been established. It is upon this fact that M. Ploucquet founded his test for determining, whether a child had been born dead or alive, by comparing the weight of the lungs with the weight of the whole body. From the experiments which he made to ascertain their proportional gravity, he drew the general conclusion, that the weight of the lungs before respiration, is one-seventieth of the weight of the whole body; whilst after respiration has commenced, it amounts to one-thirty-fifth; or, in other words, that the blood introduced into the lungs in consequence of respiration, *doubles* their absolute weight.

A third test has been suggested by M. Marc. He considers that art can never completely inflate the lungs; and, from the greater difficulty which attends the admission of air into the *left* lung, he is induced to believe, that the inferior extremity of that lung will remain in a collapsed state, and float but imperfectly, or not at all.* This test is certainly ingenious, but, I think, quite inconclusive, inasmuch as the same appearances may be observed after natural respiration has taken place; and if so, it can furnish no ground of distinction between respiration and artificial inflation of the lungs. It is a curious fact connected with the history of incipient respiration, that the *right* lung receives air much sooner than the left. M. Portal first discovered this, and afterwards confirmed it by numerous experiments. In a kitten, which he killed a few minutes after it was born, the right lung was of a whitish colour, filled the

* Manuel D'Autopsie Cadavérique, &c. p. 138.

whole cavity of the chest, and swam in water. The left was of a dark red colour, in a collapsed state, and sunk in water. He accounts for this interesting phenomenon, by showing that there is a difference in the size and direction of the bronchiæ leading to the two lobes. Upon examination, he found the right one about one-fourth part thicker, and one-fifth shorter, than the left; besides, he found the passage to the right to be more direct than that to the left.*

III. It has been objected, that "a child will very commonly breathe as soon as its mouth is born, or protruded from its mother; and in that case, may lose its life before it is born, especially when there happens to be a considerable interval of time between what we may call the birth of the child's head and the protrusion of the body."†

This objection did not originate with Dr. Hunter. It is noticed by Morgagni; and I find it discussed by the German writers early in the last century. It must be admitted, however, that the high authority of Hunter's name, has given to it an importance which it otherwise would never have possessed; and it is on this account more especially deserving of examination. It involves two points, each of which is worthy of distinct elucidation. Is it possible that a child can breathe, when nothing more than its head is delivered? and if so, is it probable that after having respired in this situation, it will die before the delivery of the rest of the body?

Although it is denied by some very respectable authors, that a child can perform the act of respiration when merely its head is born, yet the fact rests upon evidence too substantial to be contradicted. Independently of the authority of Dr. Hunter, we have several other writers who furnish us with decisive testimony on this subject. Marc alludes to a case of this kind reported by M. Siebold.‡ Capuron, a respectable French

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* Duncan's Medical Commentaries, vol. i. p. 245. Am. ed.

† Dr. W. Hunter, in the Medical Observations and Inquiries of London, vol. vi. p. 287.

‡ Manuel D'Autopsie Cadavérique, p. 140.

writer on Legal Medicine,* relates a similar instance which occurred in his own practice. And Osiander informs us, that he had witnessed twelve cases in which the child breathed and cried as soon as the head was born.† It must therefore be conceded, that a child may breathe and cry as soon as its head is delivered, although it is equally true that it is by no means a common occurrence. Admitting, then, that a child may actually breathe in the situation we have supposed, is it probable that it will lose its life before the complete expulsion of the body? That it is not, appears to me of very easy demonstration; and if so, the objection loses at once almost all its force. Even among the writers who are most strenuous in support of this objection, I have not met with a single one who pretends to have witnessed an instance in which a child has actually died in this situation. Low, although he thinks it possible, relates no case of it. Dr. Hunter, whose professed object was to enforce all the probable exceptions to the hydrostatic test, gives us nothing more than his opinion, unsupported by facts. Mahon barely admits the possibility of it. Capuron, who is sufficiently sceptical on this subject, contents himself with recording the case already alluded to, in which the child was safely delivered. Even Osiander, with all his marvellous cases, does not present us with a single one of this kind. In point of fact, therefore, there is no instance recorded, so far as my knowledge extends, in which a child has actually expired under these circumstances. This, however, does not prove that it might not occur; and it is therefore necessary to inquire into all the possible causes which might produce its death. If a child expires after the delivery of the head, and before the expulsion of the rest of the body, its death will probably be owing to one or other of the following causes:— 1. Natural debility of the child. 2. Pressure on the umbilical cord, interrupting the fœtal circulation. 3. Cessation of

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* La Med. Legal, relative a l' art des Accouchmens. Par J. Capuron, p. 405.

† New-York Medical and Physical Journal, vol. i. p. 372.

labour pains. 4. Unusual shortness of the umbilical cord. 5. A preternatural enlargement of the *body* of the child. 6. A tumour upon some part of the body of the child, mechanically interrupting parturition. I shall very briefly examine each of these in order.

That *natural debility* on the part of the child cannot occasion it, seems to be proved by the very fact of respiration having taken place; for the exercise of that function so prematurely, necessarily implies a degree of vigour inconsistent with the supposition of such original feebleness.

That *pressure on the cord* should produce the death of the child, appears equally improbable. It is perfectly plain, that when this cause proves detrimental, it must be anterior to respiration, and when as yet the life of the child depends wholly upon the fœtal circulation. In the present instance, however, the child is supposed to have already breathed; and therefore any accidental interruption in the fœtal circulation cannot, in all probability, be attended with any injurious consequences.

Cessation of the labour pains. If, after the delivery of the head, there be a sudden cessation of the pains, there is no doubt that the child may be retained in this awkward situation for some time, and that it may even lose its life before it is completely expelled. Still it must be obvious, that the chance of such an issue is very much diminished in all those cases where respiration has actually commenced, inasmuch as the performance of this function proves not merely that the child is vigorous, but also that its thorax and body are not so closely compressed by the parts of the mother as to endanger its life. Hence a child, under these circumstances, may be detained a considerable length of time, without jeopardizing its existence.

Unusual shortness of the cord. Cases of this kind occasionally occur. But here too the very fact of respiration having commenced, gives the child the best possible chance of being eventually born alive.

Preternatural enlargement of the body of the child, more especially of the shoulders, may prevent the delivery of the child, even after the birth of the head. That a child might die from this cause, is not disputed; but the very fact of its shoulders and chest being so large as to prevent delivery, shows how difficult, if not impossible it would be, for it to respire. If, however, it did actually respire, then the hazard of a long detention in this situation, would, by this very circumstance, be materially diminished. In addition to all this, the cause would here be so very obvious, on a bare inspection of the child, that no serious error could possibly arise from this source.

A tumour on the body of the child. This, of course, must be a very rare occurrence, and can never lead to any false decisions. I mention it merely because a case of this kind is recorded, in which "the head of the child was protruded, and the expulsion of the body, for a considerable time, prevented, in consequence of a large excrescence on the left breast of the child. During this interval, which was about half an hour, the child frequently cried so loud, as to be heard by the attendants."* It does not, however, appear even in this case, that the child eventually lost its life; at least, nothing is stated to this effect, in the account which is given of it. So far from supporting the objection of Dr. Hunter, which we are considering, it proves, in the most pointed and satisfactory manner, how little danger attends the child in this situation, when it enjoys the benefit of respiration.

From the foregoing discussion, it may, therefore, fairly be concluded, that, in reality, very little danger attends the child under the circumstances which we have supposed.

I shall sustain this argument by the opinions of one or two writers, distinguished for their extensive experience, as well as practical sagacity. In a case of this kind, Burns directs that we should "attend to the head, examining that the membranes

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* Mahon's Essay on Infanticide, translated by Christopher Johnson, of Lancaster.—See note by Mr. Johnson, p. 25.

do not cover the mouth, but that the child be enabled to breathe, should the circulation in the cord be obstructed. *There is no danger in delay*, and rashly pulling away the child is apt to produce flooding, and other dangerous accidents." In another place he says, "some children die, owing to the head being born covered with the membranes sometime before the body. This is the consequence of inattention, for if the membranes be removed from the face, there is *no risk of the child*."* Denman also remarks, that "it was formerly supposed necessary for the practitioner to extract the body of the child immediately after the expulsion of the head, lest it should be destroyed by confinement in this untoward position. But experience has not only proved that the child is not, on that account, in *any particular danger, but that it is really safer and better, both for the mother and child*, to wait for the return of pains, by which it will soon be expelled; and a more favourable exclusion of the placenta, will also, by this means, be obtained."†

On a review of the whole of this part of our subject, it results, that a child may occasionally breathe as soon as its head is delivered: that the very fact of its breathing in this situation, gives it the best possible chance of being born alive; and, finally, if it should even die, the cause of its death will generally be at once evident upon a mere examination of the body of the infant.

From the preceding examination of objections to the *hydrostatic* test, I think we may safely come to the following conclusions:

1. That when the lungs float in water, it must be from one of three causes: natural respiration, putrefaction, or the artificial introduction of air.

2. As the lungs may float from other causes beside respira-

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* The Principles of Midwifery. By J. Burns. Ed. by Dr. Chapman, p. 246, 376.

† An Introduction to the Practice of Midwifery. By T. Denman. Am. Ed. by J. W. Francis, M. D. p. 289.

tion, their mere floating is no proof that the child was born alive.

3. As, however, it is possible to discriminate between the floating of natural respiration and of that which is the result of other causes, it follows,

4. That, with due precautions, the floating of the lungs may be depended upon as a safe and certain test, that the child has been born alive.

I shall now proceed to examine another class of objections to the hydrostatic test, which have been much insisted upon, and which have tended, perhaps, no less than those which have already been considered, to shake the faith of the public with regard to its accuracy.

I. It has been objected that the child may have breathed, and yet the lungs, in consequence of diseases of various kinds, may have their specific gravity so increased as to cause them to sink in water. And, therefore, it is argued that the sinking of the lungs is no proof that the child was not born alive.

This objection has been deduced principally from analogy. It has been observed, that various morbid affections of the pulmonary organs of adults, as peripneumony, hydrothorax, calculi, schirri, ulcers, &c., will cause their subsidence in water; and hence it has been inferred, that the same might take place in the fœtus.

That the lungs of adults may undergo such changes from disease, as to cause them to sink in water, I am not disposed to deny, although I think it occurs less frequently than is generally believed. Haller informs us, that he has seen lungs of this description, which nevertheless continued to float. "Vidi sanguinem ex ruptis per funestam peripneumoniam vasis in pulmonem effusum, ut tamen nataret; vidi schirrhos, calculos, et lymphaticum coagulum, ut minime tamen subsideret."* He admits, however, having seen a case of peripneumony, in which the lungs sunk in water; and he adduces several other

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* Elem. Physiol. Tom. iii. p. 231.

cases of a similar nature upon the authority of others.* Heister records the case of a young man who died of phthisis, whose lungs were specifically heavier than water.† De Haen relates, that he met with three or four cases, in which portions of the lungs of adults sunk in water: and, he adds, that Diemerbroeck witnessed the same in a patient whose lungs were in a schirrous condition.‡ Hoffman details the case of a young man who died of pneumonic fever, in whose lungs the same was observed.§ In speaking of the dissections of those who died of pleurisy, in the Island of Minorca, Cleghorn says, that "in many the lungs were converted into a hard liver-like substance, and *sunk in water*."‖ Dr. Baillie confirms the same general fact. "In consequence," says he, "of the greater quantity of blood being accumulated in the inflamed portion of the lungs, they become considerably heavier, and will *frequently sink in water*."¶ Finally, there is recorded, in a late Journal, a case of "condensed lungs," occurring in a subject twenty-four years of age, and which immediately sunk on being immersed in water.** These facts are sufficient to illustrate the grounds upon which the objection rests. And they prove incontestibly, that the lungs may occasionally be rendered, by disease, specifically heavier than water. It cannot be admitted, however, that these morbid conditions will frequently occur in the fœtus, for it is not exposed to the influence of the causes which usually produce them. Haller, notwithstanding his great experience and extensive learning, relates no instance of it, and expressly asserts, that they are very rarely found in the fœtal state. "In adulto homine *aliquando*, in fetu *rarissime*, ut pulmo calculis, schirris, aliave materie, mor-

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* Ibid.

† Hebenstreit, Anthropologia Forensis, &c. p. 401.

‡ Ratio Medendi, &c. p. 114.

§ Opera Omnia, tom. 2. p. 140.

‖ Observations on the Epidemical Diseases of Minorca. By George Cleghorn, p. 159. Am. Ed.

¶ Morbid Anatomy, p. 33. Am. Ed.

** Edinburgh Med. and Surg. Journal, vol. iv. p. 301.

bose gravis in aqua subsideat, etsiquam respiraverit."* Brendel, in speaking on this subject, relates only a single case of an abortive fœtus which had schirrous lungs, and considers it a singular occurrence.† I shall only add, in confirmation on this point, the opinion of Dr. Duncan, the accomplished editor of the Edinburgh Medical and Surgical Journal. "Unquestionably a piece of inflamed lung will sink in water like a piece of liver, *but we doubt that such inflammation was ever observed in the lungs of a new-born infant*, concerning which a question of its having been still-born could arise; and we deny the fact that any portion of lungs which have breathed, will ever be rendered specifically heavier than water, by the mere settling of the blood in the lower portions after death."‡

It appears then, as well from reason as from facts, that the objection is founded upon the existence of circumstances barely possible, and by no means probable; as such, however, it demands consideration, and it is necessary to suggest the means by which a false judgment may be prevented.

For this purpose we have a test both simple and certain. The objection takes it for granted that the child has breathed; whether feebly or vigorously is a matter of no consequence. Some part, therefore, of the lungs *must* contain air, and although the quantity of it may be too small to cause the whole lungs to float, yet if they be divided into a number of pieces, and any of them remain on the surface, there cannot be a moment's hesitation about the conclusion to be drawn. Foderé states that he frequently made experiments upon lungs that were schirrous, or had congestions of blood, and he uniformly found, that although they sunk when put into water entire, yet, when cut into pieces, some of them always floated.§

II. It has been objected, that a child may have actually breathed, but yet so feebly and imperfectly, that the lungs

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* Element. Physiol. tom. iii. p. 281.

† J. G. Brendelii, &c. Medicina Legalis sive Forensis, p. 10.

‡ Edinb. Med. and Surg. Journal. vol. xii. p. 79, 80.

§ Med. Legal. tom. iv. p. 487.

shall not have received air sufficient to enable them to float: and hence it is argued, that the sinking of the lungs is no proof that the child was born alive.

In support of this objection, facts of a very pointed nature have been adduced. Heister relates the case of a very feeble infant, whose lungs sunk in water, though it lived nine hours after birth.* And a late writer on Infanticide states, that he had been informed by a physician to the Foundling Hospital at Naples, who opened daily, on an average, the bodies of ten or twelve infants, which had generally died within twenty-four hours after birth, that he hardly ever found more than a very small portion of the lungs dilated by air: this portion was frequently not larger than a walnut in its green shell, and but rarely larger than a hen's egg, and it was commonly situate in the right lung.†

The same method must be here adopted, as in cases where the lungs are diseased: they must be cut into several parts, and experiments instituted upon each. However imperfect the respiration has been, some portion of the lungs will necessarily be inflated, and therefore must ~~sink~~ float.

III. It has been objected, that a child may be born alive without breathing; and therefore, in these cases, the sinking of the lungs is no proof that the child was not born alive.

The fact upon which this objection is raised, cannot be questioned; nevertheless it is both safe and just to consider as dead every child that has not breathed. Governed by such a rule, any error that may be committed will always be on the side of mercy. It is true, that certainty is as desirable here as in any other case; for the destruction of a feeble child is a crime as enormous as that of a vigorous and healthy one; and the punishment of the murderer of the one, is equally an object of public concern with that of the other. But, in the language

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* Morgagni's Works, vol. i. Lett. xix. p. 536.

† A Dissertation on Infanticide, in its relations to Physiology and Jurisprudence. By W. Hutchinson, M. D. 1820.

of a distinguished writer on this subject, "pour le punir, il faut le constater; et lorsque les limutes de l'art nous refusent le degré de certitude que nous ambitionnons, la clémence, que dis-je, la crainte d'immoler l'innocence devra l'emporter sur toute autre consideration."*

This objection, so far from showing the inconclusiveness of this test, serves only to establish more clearly its absolute necessity. It is by resorting to it alone, that the sacrifice of innocence can be prevented: for who would assume the responsibility of deciding that a child had been born alive, when no evidence could be discovered of its having respired?

From the foregoing considerations, it may therefore be concluded,

1. That when the lungs sink in water, it must be from one or other of the following causes: the total want of respiration; feeble and imperfect respiration; some disease of the lungs rendering them specifically heavier than water.

2. As the lungs may sink from other causes than the absence of respiration, their *mere sinking* is no decisive proof of the child's having been born dead.

3. As, however, the sinking from the want of respiration may easily be distinguished from that which is the result of other causes, it follows,

4. That with due precautions, the sinking of the lungs is a safe test that the child was not born alive.

I have now gone through the discussion of this subject; and although the general conclusion is decidedly in favour of the accuracy of the hydrostatic test, yet nothing can be plainer than the necessity of an extensive acquaintance with the subject, to enable the professional witness to do justice to himself and to the cause of truth. It is much to be feared, that from the ignorance of some, and the precipitancy of others, great and fatal errors have not unfrequently been committed. It may not, therefore, be improper to present a summary of *practical rules*, for the guidance of the physician when called to

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* Manuel D'Autopsie Cadevérique, par C. C. H. Marc, p. 140.

the examination of a case, which, of all others, demands a combination of the exercise of the soundest judgment, and the most profound knowledge.

1. As preliminary to any examination of the lungs, the child should be weighed, and the general appearance and condition of the body should be particularly noted, with the view of ascertaining the following points, viz.: if the child be full grown; if the different parts of its body be well proportioned; if the shoulders be uncommonly large, when compared with the size of the head; if any tumours are to be found upon the body; if the cord be unusually short; and, finally, if any symptoms of putrefaction be present.

2. The chest should then be carefully opened, and the following things noticed: the general shape of the thorax, whether it be much arched or otherwise; if the lungs be collapsed or dilated; whether they cover the lateral parts of the pericardium and heart; whether their colour be deep red or lighter; whether there be any appearance of disease or of putrefaction.

3. The next step is to remove the contents of the chest, for the purpose of performing the necessary experiments upon the lungs. The aorta and vena cava should first be tied near the heart, and then cut beyond the ligatures: the trachea should then be also divided: the lungs, together with the heart, are now to be taken out of the chest, and to be submitted to an additional inspection, to ascertain whether they are sound or diseased, and if they are at all affected by putrefaction.

4. A convenient vessel containing water should now be provided; and particular attention should be paid to the temperature of the water, in which the lungs are to be immersed. The reason of this will be perfectly obvious, when it is recollected, that the specific gravity of water varies with its temperature: thus, for instance, water at 100° is lighter than water at 60°, and still lighter than at 40°. Besides, if the water be too hot, it will have the effect of expanding the lungs, and thus favour their floating, especially when there already exists a tendency to putrefaction. If, on the contrary, its temperature be too low, the air cells may be contracted, and much of the air be

thus expelled. The temperature of the water should therefore be regulated by that of the surrounding air. Another precaution relative to the water is, that it should not be impregnated with *salt;* for, in consequence of the greater specific gravity of saline water, a body might float in it, which would sink in fresh water.

5. The lungs, together with the heart, should then be cautiously placed in the water, and it should be observed whether they float or sink: if they float, whether above the surface of the water, or just under it; if they sink, whether they do so rapidly or gradually.

6. The lungs should then be taken out of the water, and after tying the pulmonary vessels, they should be separated from the heart, and accurately weighed.

7. The lungs should then be replaced in the water, to see whether they sink or float, and in what way.

8. The two lobes should then be separated, and the same experiment repeated upon each, noticing the difference, if any, between them. If only one floats, see if it be the *right* one.

9. Each lobe should then be divided into a number of pieces, taking care not to confound the fragments of one lobe with those of the other; and upon each of these the same experiments should be instituted.

10. While cutting the lungs, observe if there be any crepitus; if the vessels are charged with blood; and if there be any traces of disease.

11. If any of the sections of the lungs float, they should be taken and squeezed forcibly in the hand, and then replaced in the water, to determine whether after this they will sink.

After having performed these different processes, the conclusions to be drawn from them are evident. If there is nothing to be discovered on the body of the child, to favour the belief that it might have lost its life during delivery; if the lungs be not touched by putrefaction, nor be artificially inflated; if on cutting into them, a crepitus be perceptible; if the entire lungs, as well as all the separate divisions of them, remain on the surface of the water; if, after squeezing portions of the lungs,

they still continue to float: then the mass of evidence is irresistible, that the infant was born alive, and enjoyed perfect respiration. If only the *right* lung, or its pieces, float, the respiration has been less perfect. If some pieces only float, while the greater number sink, it proves respiration to have been still less complete. On the other hand, if neither the entire lungs, nor any section of them, float in water, the inference is decisive, that the child never respired. Several other inferences of value are to be deduced from the foregoing experiments; but as these must be perfectly obvious, after what has been stated in the former part of this paper, it is deemed unnecessary to enter into any farther details.

ART. VII. A CASE of ABORTION, *at the sixth month, with attachment of the Placenta over the os uteri.* Communicated to Professor ANDERSON, of this city, *by* ROBERT BAYARD, M. D. of Horton, Nova Scotia.

MY DEAR SIR,

Enclosed you have the summary of a case that occurred to me in practice, which, should you think worth a place in any of your periodical journals, you will please to have it inserted.

Among the various perplexities which occur in obstetric practice, there is none that has excited greater alarm and embarrassment, than the attachment of the placenta over the os uteri. The hemorrhage which follows the parturient action of the uterus, has occasioned a promptitude of practice, which, notwithstanding the apparent danger, seems to have originated more from apprehension than the actual urgency of the case, when anatomically considered. It is true, the danger is great too great, to admit of quietly awaiting the efforts of nature. Such indifference would prove fatal to the patient, in the great, majority of cases, as instances have occurred as exceptions, in which the child and placenta have, under such an attachment, been both expelled, and the woman survived; not, however, without a loss of blood, which induced an alarming exhaustion; but, it must be observed, that in the instances of

this kind, which came within my own knowledge, the women were unfortunately attended by unskilful practitioners, who were not aware of the proper means of restraining the violence of the hemorrhage.

It has been generally recommended, when the placenta is implanted over the os uteri, to expedite labour; and, as soon as the os uteri is partially dilated, to force the hand through it, lacerate the placenta, rupture the membranes, bring down the feet of the child, and effect delivery as speedily as possible. Now, I object to this, in toto, as a general rule of practice; and, I suspect, that, in many cases, it would increase the difficulty and danger. It is certain that it always increases the sufferings of the woman, and is seldom, if ever effected, without a most distressing loss of blood. I have delivered patients by this violent interference, and have witnessed scenes of anguish, flooding, and debility, which imperiously point out the necessity of ameliorating the practice in such deplorable cases. Anxious to effect an improvement so essential, I was determined to adopt a different mode of procedure, in the first case that should come under my charge. The following observations, I trust, will induce other practitioners to repeat proceedings which were successful, and seem justified by the anatomical arrangement of the parts.

Mrs. Ebenezer K——n, the wife of a respectable farmer, and the mother of a large family, was subject to frequent miscarriages about the fourth month; which had induced a leucophlegmatic appearance, and general as well as uterine debility. She was particularly disposed to menorrhagia, especially at the first monthly period after her confinement, whether regular or premature; she suffered severely from excessive flowing. I was called to attend her in the beginning of January, when I found her losing blood freely from the os uteri; she was then four months pregnant; complained of little pain, but more of a sensation of uneasiness across the hips; there was no expulsive effort of the uterus; the bowels were costive; the countenance pale; the pulse feeble; the sanguineous discharge from the uterus was reduced in quality by a copious

admixture of water, which the patient referred at first to the rupture of the membranes. The bladder was but slightly affected. From the appearance of symptoms, I was disposed to attribute the discharge partly to over exertion on the part of the patient, who was an industrious and hard-working woman, but principally to a morbid habit of the womb, induced by previous miscarriages. I accordingly enjoined strict rest, the recumbent posture, and gave liberal opiates, the gentlest locatives, and ordered the use of cold astringent applications, with the employment of plugs, as recommended in early abortions. This plan was pursued for several days, during which the discharge continued. This was however unquestionably owing to the very imperfect manner in which the vagina was plugged. The patient in a few days became somewhat feverish, which I ascribed principally to a costive habit of body, but fearing there might be some uterine congestion, I determined, in spite of the contra-indicating appearance of her countenance, and weakness of pulse, to draw some blood, which I did; but a few ounces satisfied me of the inutility of it. The first plan was therefore more rigorously enjoined. I examined the os uteri in the first instance and found it not sufficiently opened to admit the end of the finger. As I could not pass it within the womb, I was disposed to attribute the discharge to congestion and the rupture of some vessels about the cervix or os uteri. As the patient could but poorly sustain a further loss of blood, cold astringent injections were thrown up, and the plug again introduced; this seemed to do well for a few days. The debility of the woman was so great as to demand moderate cordials and tonics; the camphoretted emulsion, the tincture of cinchona, and moderate port wine were accordingly allowed; her countenance and strength seemed evidently improved by them. The discharge of blood was less, and its watery proportion greatly reduced; the discharge at times had an unpleasant smell, which led the woman and her friends to suspect that the child was dead. But the patient soon changed her opinion from a consciousness of the movement of the

fœtus in utero. The copiousness of watery admixture with the blood was such for four or five weeks, as to excite reasonable suspicions of an hydropic affection of the womb; which seemed strengthened by the general appearance of the woman. Eight weeks elapsed with occasional alternation of symptoms.

February 26th—I was sent for, and found her labouring under manifest indications of abortion: she had lost much blood during the night; her pulse was very quick and feeble; she was faint, and had occasional imperfect labour pains, which were characterized by a sensation of anguish, rather than of bearing down. I examined the os uteri, and found it dilated in diameter about two-thirds of an inch, and perceived the placenta implanted over it; the vagina was filled with coagula. I could pass my finger within the os uteri, and satisfactorily feel the adhesion of the placenta to the uterine surface, from which it was detached about an inch all around the os uteri. I was now sensible the discharge came from this divided portion: whether it issued equally from the placental and uterine vessels, I am not prepared to say. I was aware that no blood could escape into the uterus, which was not only already filled completely with its fœtal contents, but was guarded from such an influx by the yet adhering placenta. I was also sure, that if the vagina was thoroughly secured, and its cavity plugged, that very little blood could be lost in that cavity: my chief desire was to prevent the effusion of blood, and promote the expulsitory efforts of the womb. I considered, that the further loss of blood to any considerable amount in the reduced state of my patient, would have been fatal to her: and I also anticipated some difficulty in forcing the womb at this period of gestation. I accordingly plugged the vagina, and kept an attendant pressing against the labia vaginæ with a soft towel folded as a compress, so as effectually to prevent the discharge of any blood. I gave cordials; and by friction on the abdomen promoted, as far as I could, uterine contraction: the pains were now slight: three hours passed in this manner, during which there was little or no discharge, excepting when I introduced the finger to ascertain the progress of

the labour. My patient did not grow sensibly weaker. In the course of four hours, the efforts of the womb became more frequent and strong, so that I found the detached placental portion protruding through the os uteri; while I at the same time could distinctly ascertain its adhesion within the uterus, now about a finger's length from the dilated margin: as the portion protruded, I found I could insinuate my finger into its lobulated texture without lacerating afresh any more vessels, and plainly feel its membraneous lining. I now kept my finger constantly within the vagina, favouring as much as possible the dilatation of the os uteri. I at the same time guarded against the hemorrhagy, which I found would otherwise ensue, by compressing the labia vaginæ *firmly* together with my thumb and other fingers, whilst the index finger was within the vagina. This was easily and advantageously managed. The pains now assumed a determined character; and, as they were increasing, I watched my opportunity and ruptured the membrane; the liquor amnii gushed out copiously; a strong contraction took place; the volume of the uterus was diminished; the disposition to hemorrhagy abated; the placenta passed into the vagina, and in a few minutes the fœtus was expelled, completely enveloped with its placenta. The disposition to flooding of course ceased; and my patient was delivered with comparatively little pain, and very little discharge from the time the plugs and compress were firmly employed. It may possibly be observed, that the practice adopted in the foregoing case might answer in premature labour, but be inapplicable at the full period, when the blood-vessels are much larger, and the hemorrhage consequently more copious. In reply, I must remark, that in the above instance the placenta was unusually large, and the circumstances altogether of such a nature, as to convince me of the expediency of repeating the same means at any period, until careful experience invalidates or condemns them. If we advert to the relative thickness of the uterine parietes at the sixth month, and full period of gestation, the danger from hemorrhage, *cæteris paribus*, would be most urgent at the sixth month; because, at the ninth month, the substance of the ute-

rus is thickest, and when contraction occurs, especially after the liquor amnii is evacuated, it allows the divided vessels to shrink into that substance: moreover, the pains are generally more powerful at this period, and the muscular efforts of the womb compress its lacerated vessels more forcibly, and thereby lessen the tendency to flooding. If strong pains, and increased thickness of the parietes of the womb, are favourable in repressing hemorrhage, weaker pains, and proportionate thinness of texture, must conduce to an increased discharge. That the parieties of the womb are thinner at the sixth than at the ninth month, no person will deny: and if the force of contraction in this case, is in the ratio of the size of the muscle, which I think is also indisputable, the uterine vessels are less liable to be compressed, and are, of course, more disposed to copious bleeding. To this imperfect contraction of the womb, and consequent diminished compression of its vessels, I think we may frequently refer the disproportionate hemorrhage which occurs in the early months, when a large quantity of blood flows freely from a comparatively small surface. It may be said, that a woman suffers pains as strong in the early months of miscarriage, as she does in regular labour. I can only say, that in a variety of cases which have come under my charge, I have never observed them. I admit, that she often suffers more from abortion than regular labour. But the suffering is not attributable to the strength of the pains: it is anguish, referable rather to the occurrence of a process in an irritable or irritated constitution, unprepared by nature for any such morbid action. I have now to request of you, or any of your friends, a candid opinion, with remarks, on the subject of the above case.

Yours,

ROBERT BAYARD.

Art. VIII. Diary *of the Occurrences of the first month of the* Yellow Fever *which prevailed in the City of New-York in* 1822, *with facts and observations relative to the nature and character of the disease.* By Daniel D. Walters, M. D. of New-York.

New-York, Dec. 17, 1822.

TO THE EDITORS OF THE

NEW-YORK MEDICAL AND PHYSICAL JOURNAL.

Aware of the deep solicitude of the public on all that concerns the late pestilence, I have offered for publication in your Journal a detailed account of such facts as may illustrate the origin and nature of the dis ease. Less minuteness would have defeated the object I have in view. The present paper is intended to be submitted to the consideration of the Board of Health of our city.

With sentiments of respect,

I remain,

D. D. WALTERS.

To Drs. Francis and Beck.

TO THE

HONOURABLE STEPHEN ALLEN, ESQ.

MAYOR OF THE CITY OF NEW-YORK, AND PRESIDENT OF THE BOARD OF HEALTH.

Sir,

I have written a Diary of the first month of the late fever, as it appeared in this city, with some facts and observations relating to the disease. Besides attempting to show whence the fever of this year originated, I had other objects in view. One of these was to throw in my mite towards the correction of public opinion on this subject; another was to prove that the Board of Health, as now appointed, is not organized on the best possible plan, and that the laws which regulate it, as well as those governing the quarantine establishment, are partly inadequate to the object, and more or less founded on erroneous principles. Another object to be answered by this communication, was to show that although the Board of

Health could not be officially informed of the existence of pestilence until a month after its appearance; yet that some persons did know the true character of the disease at its beginning. If this had been the first occurrence of the kind which had fallen under my own observation, I should most likely have remained silent on the subject. The same difficulties have happened in a greater or less degree several times before, and are even likely still to occur, until some radical change takes place in the organization of the Board of Health, and in the laws regulating the quarantine establishment.

Since the year 1798, I have thought a communication of the kind here presented, due to the public interest: for in that year the same difficulties took place, with consequences shocking to humanity, and which beset the Board in so remarkable a manner during the first month of the late fever. The Board of that year, while feeling no doubt the most laudable concern for the sufferings of their fellow-citizens, and actuated by the best motives for the public good, could not become officially informed of the existence of pestilence in the city, until its ravages had spread far and wide beyond its control: its exertions were therefore of course paralyzed, and it did nothing except passing a resolve, for quieting the great alarm which existed at that time, "*That the city was free from yellow fever.*"

In order to bring the whole ground of difficulty before the public in the way best calculated for its correction, I have thought it best to proceed in the manner I have done, that is, to give a detail of all the facts as they occurred daily, in connexion with my own relative proceedings, and for whatever may appear to savour of egotism, my only apology is, that the public good seemed to require it. These sheets I now beg leave to submit to you, and through you to the gentlemen who compose the Board of Health; because I have used some of their names with a little freedom. I have quoted some conversations which took place between them and myself. If I have misquoted any thing, I hope they will be so kind as to correct it, for with that intention it is laid before them; and if by any thing that I have written, the interests of this city can

be promoted, I shall consider my feeble exertions to be well rewarded.

I have, Sir, the honour to be
Your most obedient,
and humble servant,
DANIEL D. WALTERS.

New-York, Dec. 17, 1822.

On the evening of Thursday, the 11th of July, 1822, I visited Mr. Reder's daughters, Amanda and Caroline, the former aged eleven, and the latter nine years. I found them both labouring under the general symptoms of a violent fever. Their mother informed me that they had sickened on the preceding evening. Mr. Reder resides in a neat, airy, brick house, two stories high, situated at the west end of and fronting Rector-street, one door from Washington-street. This spot, in common with all the west side of the city, was always, until the present season, justly considered remarkably healthy, and, indeed, its local advantages in favour of health are certainly very great, as it lies entirely open to the cooling and ventilating operation of the southerly and westerly winds which prevail nine-tenths of the time during our summer months.

Friday the 12th.—I found my patients without any abatement of fever.

Saturday the 13th, was a rainy day, and being myself indisposed, I therefore did not visit them.

Sunday the 14th.—I saw them at 10 o'clock in the morning, and found that all their feverish excitement had entirely subsided; their eyes, necks, and breasts had become tinged with yellow, and Caroline had black vomit. At this juncture it was not easy for one who had marked the symptoms from their commencement with fever to their present malignant change, to be mistaken with respect to the nature of the disease: I therefore in a guarded and private manner, expressed

my opinion to a few of my friends, for at this time I did not mean to give the alarm, in anticipation of the measures of the Board of Health, nor even to name the disease to the public.

Monday the 15th—I found Amanda much as on the 14th, but Caroline appeared to be fast approaching to the fatal termination of her malady. After visiting my patients on this morning, I called on Mr. Waldron B. Post, with whom I had some business. He resides in Greenwich near Rector-street, I informed him that the yellow fever had appeared in his neighbourhood, and that the safety of himself and family required his attention to the circumstance; which I believe he immediately did, by looking out without delay for a place of retreat. Towards evening on this day, in a walk down Vesey-street, I met with Doctors Hosack and Francis; I informed them that yellow fever had broken out in Rector-street. Dr. Hosack inquired, whether I had reported it to the Board of Health. I answered in the negative: he replied, "If I knew it to be yellow fever, I would report it as such."

Tuesday the 16th.—On the morning of this day I found Amanda convalescing, and Caroline growing worse; but John, an interesting youth of seventeen years of age, had sickened during the preceding night: from the violence of the first symptoms I judged that his case would in its course become highly malignant. On my way from Rector-street, in Broadway I met Dr. Daniel W. Kissam, in company with a gentleman, with whom I was unacquainted. I informed them that yellow fever had made its appearance in Rector-street. I now proceeded directly to the City Hall, where I reported the cases nearly in the words of the following conversation, which took place between his honour the Mayor and myself:

"Sir, there are three cases of fever under my charge, which I think demand the attention of the Board of Health."

"What are the names of the persons sick, and where are they to be found?"

"They are the children of Mr. Reder, who lives in Rector-street, next door to the corner of Washington-street."

"What kind or description of fever do you think they have?"

"I think it such a fever as our soil and climate are not calculated to produce; a fever depending on the introduction of foreign poison for its origin, and exotic in our country, and such a fever as is likely to do great injury, if it be allowed to spread; and I wish the Resident Physician may be requested to examine the patients and name the disease: and I further wish, that neither Dr. Hicks, nor any of the assistants of the Board, be permitted to visit them at present; if, however, you think it would be agreeable to the Resident Physician, I will wait on him, and introduce him to the patients."

The Mayor replied, "I will write him a note to that effect, and request him to await your call at his own house at four o'clock this afternoon."

Agreeably to the above arrangement, I called, in company with Dr. W. Miner, on the Resident Physician at four o'clock, when the following conversation took place between us:

"Doctor, your patients in Rector-street have bilious fever."

"Have you seen them, Doctor?"

"Yes, after receiving notice from the Mayor this morning, I happened to be in that part of the city, and I thought it more convenient to visit them before dinner."

"Well, I am glad you found the disease nothing worse. I fear, however, it will give you some trouble before you have done with it."

After some other observations on the subject of our difference of opinion, I left the doctor, not at all offended at his having visited my patients at an earlier hour than had been suggested to him by the Mayor.

Wednesday the 17th.—On this day Caroline died, and John grew worse. The Board met at 2 o'clock, when the Resident Physician stated, "that he had no report to make." He had, to be sure, visited my patient in Rector-street, and found them ill with *bilious*, but not yellow fever. Finding the Board about to take no measures on the subject, and that, of course, not only my friends, but also the public in the neighbourhood

of the disease, would be exposed to the danger of a spreading pestilence, I resolved at once to throw off the caution, which I had hitherto observed, and state, to every inquirer, my opinion on the whole matter. I accordingly lost no time in declaring to every member of the Board, with whom I was acquainted, and to all its assistants, that the disease in question was positively YELLOW FEVER. Among the members thus informed, were his Honour the Mayor, Aldermen Fairlie and Hall, Doctor Dyckman the Health Commissioner, General Morton the Clerk of the Board, and the Assistants, Doctors Cutter and Hicks, and Captain Mills. Three days after this, Alderman Taylor, of the eighth ward, with whom I was unacquainted at the time, learned my opinion of the disease, in a conversation which he commenced in the Mayor's office, as follows: "Doctor, we think it very strange, that you should tell your friends that there is yellow-fever in Rector-street, when you have not reported it as such to the Board." My reply was, "I mentioned the existence of this fever to the Mayor, on last Tuesday morning, in such terms as, I thought, might call the attention of the Board to it; but, in that report, for particular reasons, I purposely omitted the use of the word *yellow*; but I now tell you, that it is positively the yellow fever, and, knowing it to be such, I consider it my duty to inform my friends of the fact, that they may be enabled to take care of themselves in time. I am aware that the Resident Physician is of a different opinion, but it is immaterial to me, as an individual, what either he, or the Board, may call the disease. We think very highly of Doctor Quackenbos; he was very correct in the year 1819. As a man, I think very highly of him too; but my conduct in this affair must be governed altogether by my own opinion of the nature of the fever in question."

Thursday and Friday the 18th and 19th.—John's symptoms progressed from bad to worse, until Saturday the 20th, when black vomiting began. On the morning of this day the Resident Physician visited him, in company with Doctor Manley, who started a new opinion of the nature of the disease, by calling it "Savannah fever," which, by the by, is a

species of bilious fever, that is never communicated from vessels or goods to our shores, so as to become located, and to affect the inhabitants. But, by some means or other, it happened that Doctor Manley was employed to attend John with me, as consulting physician. We saw him together at 12 o'clock, and I observed the Doctor began to think his morning opinion not tenable; and, at his evening visit, he stood perfectly corrected; for, as we left Mr. Reder's house, we met Mr. Noah, the editor of the National Advocate, at the corner of Rector and Greenwich streets, who made some inquiry of us touching the nature of the fever in the neighbourhood; to which Doctor Manley replied in these words: "If I was obliged to give an opinion of this disease, under oath, I should say it is yellow fever;" and to an observation of Mr. Noah, "we are in danger here, if that is the case," I answered, "Yes; while standing on these paving stones, you may consider yourself as knee-deep in pestilence." On Monday the 22d, John died. The only family, besides that of Mr. Reder, living in Rector, between Washington and Greenwich streets was that of Mr. De Lange. This man I advised to leave the neighbourhood, which he did on the 22d, but not early enough to escape suffering, as one of his sons had the disease severely, after retiring to Long-Island. A few days after this time, the Board met with closed doors, in the small room adjoining the Mayor's office. I happened to be in the hall at the time, and was invited into the room before the assembled Board. The Recorder expressed a wish to hear what I had to say on the subject which had called them together. I stated, that "I had been compelled, by the symptoms of the disease in Rector-street, to consider it precisely that variety of yellow fever, which overran this city in the year 1798."

During my attendance on Mr. Reder's children, after much inquiry, whether any other person had sickened in the neighbourhood, I learned that Andrew Thomas, a young Scotsman, had been sent sick, on or about the 11th, from the south-west corner of Washington and Rector-streets, to the New-York

Hospital, where he died on the 16th. I mentioned these circumstances to the Mayor, who caused inquiry to be made, from which it was clearly shown, that Mr. Thomas did die of strongly marked yellow fever in that Institution, without greatly disturbing the quiet of its managers.

As the Board seemed to feel itself bound to consider all information, not derived from the Resident Physician, incorrect or unofficial, it of course continued to act in an undecided and hesitating manner, until the 5th of August, when the alarming progress of the disease forced that officer to acknowledge its true character. Dr. John Neilson, to be sure, after a little disputation with the Board about that unmeaning word *bilious*, did, on the 31st of July, report two cases of this disease as yellow fever, the symptoms of which he well described in a letter to the Board on the following day. His report, however, as it did not come from the right source, like the declations which for two weeks previous I had daily made on the same subject, was insufficient and unavailing towards influencing the decisions of that assembly.

The number of cases continued to increase daily until the 9th of August, when the Resident Physician seemed to think that the public mind demanded some little explanation, in order to enable it to account, in the most reasonable manner, for what was going on. This explanation he made in the following words: "From the above facts it appears evident, that the cause or causes, which at first were only sufficient to produce bilious fever, have now become so concentrated as to create yellow fever." This puzzling and unaccountable concentration of the cause or causes of this disease, if it really took place, was certainly a manœuvre calculated to mislead the most discerning. We have, however, good reason to doubt the fact; as the number of deaths, before the announced concentration of the cause or causes, was fully equal to what happened afterwards, in proportion to the number who really had the disease.

From this time, the Board met daily until late in October, manifesting the greatest solicitude for the public welfare; for

all parties acknowledged that yellow fever really existed in town. The table of their chamber now daily teemed with the speculations of the learned, on arresting the progress of the disease, or destroying its cause. These propositions, however, to say the least of them, came six weeks too late; for by this time the infection had spread over a surface by far too wide to be controlled by human power. A general removal, which took place from the lower parts of the city, was the only means of safety left the inhabitants; and the disease continued to spread until the 20th of September, when a very heavy rain occurred, followed by a cold, strong, northerly wind, of two days continuance. The effect produced on the progress of the fever, by this rain and wind, was, in a high degree, striking and sensible. The out-of-door poison, or that part of it which had possession of the streets, seemed to be entirely destroyed; the extension of the infected district, which until this time had been gradual and well marked, ceased; and, in four or five days, the cases diminished, until the eighth after the change of weather, when not one was reported to the Board. Shortly afterwards, however, the poison again showed itself; but the subsequent cases appear to have arisen from infection remaining in the houses, and not to have been acquired in the streets, as was frequently the case before the storm.

Having given, above, as much of the late fever, in connexion with the relative proceedings of the Board of Health, as I think due to the public interest, the public curiosity, or to my own reputation, I shall proceed to the consideration of the more important parts of my subject: and the first and most important question to be answered, is, Where did the poison which caused the fever of this year, come from? This question has been, and will continue to be, answered by the advocates of different theories, in such a way as to give the most plausible support to their favourite opinion. The exclusive believers in home or local origin, will ever find an old sink, a cess-pool, a rotten potato, or a putrid mouse, or some other nuisance of the kind, sufficient, if one could believe their stories, to poison a world. These gentlemen, however, do not seem

to recollect. that the nuisances, from which they fear so much, are the hourly productions of nature, and abound every where, at all times, over all the earth, rarely causing disease among its inhabitants. I am willing to admit, that under peculiar circumstances of confinement, or want of ventilation, accumulations of filth may excite typhus fever of a very malignant type: I, however, do not admit, but assert the contrary, that the exhalations from any or all these nuisances have the least influence on yellow fever, either to render it more or less malignant, or to retard or hasten its progress; for yellow fever is for ever precisely the same disease, with its power to kill unimpaired, whether it appears in the healthy atmosphere of Middletown, in Connecticut; the Wallabout, Brooklyn; the Quarantine Ground, or the more impure atmosphere of this and other large cities; and all the stories, even admitted as true, related of churches, cemeteries, sinks, jails, and processes of putrefaction, are not worth a jot on the present question: they prove nothing; as the relaters of them have to show that they ever, in a single instance, have caused yellow fever. On the question of cleanliness, they are, however, of inestimable value, as they prove the absolute necessity of rigidly enforcing the observance of that virtue. Dirty streets, yards, and damp, filthy, unventilated houses, not only originate several varieties of fever, but sensibly aggravate most diseases to which the human frame is liable; and when these accumulations of filth happen to be assisted by the most vicious habits which idleness and drunkenness can engender, they frequently cause such a fever as appeared in Bancker-street two years ago, which killed negroes by the dozen, and desperately pothered some gentlemen of the faculty, setting them agog for endemic yellow fever.

Church, jail, and Bancker-street fevers are always caused by a poison, generated and existing within the apartments in which the sick are found; and to remove the cause and restore salubrity, it is only necessary to cleanse and ventilate. But with yellow fever the fact is otherwise; this is always caused by an out of door poison, which first gains possession of the streets, from which it enters with equal facility the palaces of

the rich and the hovels of the most wretched; carrying to both the same power to destroy. Ventilating and cleansing are here of no avail, until the stormy winds and low temperature of approaching winter have driven the enemy from the streets.

New-York has the reputation of being a dirty city, or at least, it has to my knowledge been constantly complained of as such for the last twenty-five years; some parts of it are, however, infinitely more dirty than others. The Collect, and Swamp, for instance, may be said to be natural cess-pools of very great extent, each of them containing many acres of made ground, and each of them receiving, from time immemorial, as in a place of deposit, the wash and filth of a large surrounding surface. Yet these places, although ten times as filthy as any other part of the city, have always been remarkably exempt from yellow fever. This fact is, however, easily accounted for, in the uniform manner of attack pursued by the disease in question, which is invariably on the wharves, by the side of vessels, and no where else: hence, it evidently appears, that if our city filth has any agency in producing yellow fever, it is only the filth on the wharves; for all the experience we have on the subject goes to prove, that this disease never made its first appearance in the interior of this or any other northern city.

Before meddling with that fruitful bone of contention among the different theorists who speculate on this subject, to wit, the consideration of the causes in and about Rector-street, whether of foreign or domestic origin, which could have contributed to the production of the late fever, I will proceed to make a few observations on what I deem an incorrect and unphilosophical habit indulged by too many, in forming their judgment of things altogether from external appearances, or on the sensible qualities of taste and smell. This habit appears to be partly natural and partly acquired, but regulated in some cases by our experience. Toads and spiders are charged by thousands with a high degree of venom, merely because their Creator, in his wisdom, did not think proper to give them an aspect corresponding with our notions of beauty:

and even with our ample experience of the perfectly harmless natures of these little animals, we cannot look at them without feelings of hostility : and a man ignorant of the effects of aloes and arsenic, would, on account of its offensive taste, throw the former from his mouth, as poisonous ; while, without suspicion, from its insipidity, he would readily swallow in a dish of tea enough of the latter to destroy him. In no way, however, has the mind been warped by prejudice from correct and just conclusions, so strikingly and so frequently, as where offensive impressions have been made on the organ of smelling. This has been more particularly the case in this city for the last twenty-five years. Prior to the commencement of that period, the people, like the inhabitants of the country now, could approach the offensive atmosphere of a putrid carcass, a mud hole, or a heap of manure, without ever once thinking of yellow fever. The frequent introduction, however, of the invisible poison, which causes that disease, in a way unaccountable to such as would not be at the trouble of acquiring correct information, has led thousands, and among that number some physicians, to ascribe this fatal malady to the nearest offensive object of the nuisance kind, that happened to exist at the moment on a neighbour's premises. For, curious as it may appear, I have never yet been able to find a man who would acknowledge any nuisance about his own house, however offensive, capable of producing disease ; but when his family sickened, it was caused by spoiled provision in his neighbour's cellar, or by a mud-hole, or a broken sink on an adjoining lot. These people never once think of correcting their opinions by past experience. If they did, by referring back to the half century preceding the year 1798, they would learn that any or all these nuisances were never so mischievous in those days as to cause yellow fever. Previous to the year above-mentioned too, no regulations of the least use had been adopted for keeping the city clean, and nothing was more common than to see dead cats, dogs, and even hogs, broken up by the cart-wheels in every part of the streets. During a visit which I made a few days ago to the neighbourhood of Rector-street, I could

not but observe the application of the foregoing reasoning to the particular subject under consideration. The inhabitants of that vicinity appeared to be divided into four classes, three of which were altogether *led by the nose*, to make answer to this question, which I put to a number of highly respectable gentlemen: "What do you think caused the late fever among you?" First class, "We think it must have been caused by the cess-pool, for it emitted a very bad smell." Second class, "We think it must have been produced by some puddles of water and filth, which stagnated in the gutters, and became, from the heat of the weather, very offensive to the smell." Third class. "We have no doubt but that it originated from the contents of the sinks, which were thrown into the street about that time, for the stench of them was intolerable." And a fourth class, who had not been seized so unceremoniously by the nose, believed it to be derived from vessels or goods brought to the wharves.

So great is the disposition of many, to ascribe mischievous powers in producing disease to all substances of strong and offensive scent, that I should not be surprised to find some hereafter ranking assafœtida, hepar sulphuris, and other medicinal drugs, among the causes of yellow fever. It is, however, certain, that yellow fever has no necessary connexion with sensible smells; for allowing the poison, which causes that disease, to arise from nuisances of any kind, in a state of mixture with the materials of stench, the combination exists only for a very short time, as after every thing sensible to the nose has been driven off, and dispersed by the wind, the infection will continue for months to spread in every direction, as inodorous as it is invisible.

The sources of mischief, in or near Rector-street, which have been spoken of by the different parties to this question, as having a supposed agency in causing the late fever, I shall divide into two kinds: first, those on shore, of local, or home production; second, those at the wharves, derived from goods or vessels, and of foreign origin. Of the former kind there are three; and, at the head of these, stands the cess-pool.

As this is a word only very lately brought into use, and in bad company too, it is necessary to give a very particular description of the thing signified by the name, in order that no reader may attach to it some magical meaning of horrid import. A cess-pool is, then, nothing more nor less than a little, paltry, dirty, artificial mud-hole. The cess-pool, now to be particularly described, was situated two or three feet from the south end of an alley, which runs north from Rector-street, on the east-side of Mr. Reder's ground, and across the rear of three lots which face Greenwich-street. It was placed there four years ago, when Rector and Greenwich-streets were raised above the adjoining land, to receive the wash and filth of the four lots bounding the alley in which it is contained. It was three feet deep, and four feet in diameter; planked up around the sides, and covered with an iron grate; and, like all other mud holes, it was sometimes, during the heavy rains, filled to overflowing. Five months previous to the beginning of the fever, it was cleaned out; and, about the beginning of July, the water which it contained was taken from it, and thrown into the street. Of the stench of this nuisance I have spoken above; some of the inhabitants complained of it; others did not. Some time, during the last week of July, it was particularly examined, by order of the Board of Health, by Doctor Hicks, and two other persons, and found, though not very offensive, to contain a quantity of mud which had not been disturbed, when the water was removed a month before. This mud was taken out by two men, put into a cart by a third, and the hole filled up by a fourth. Of the seven persons employed about this affair, not one of them contracted the fever.

Before touching particularly upon the second source of offensive matter, which some persons have spoken of as having a supposed agency in producing disease, viz. several new and old sinks near Rector-street, it is necessary to say something of the nature of the ground in which they are located. All this shore of the city, west of Greenwich-street, is what is commonly denominated "made ground;" that is, it has been raised above the flowing of tide-water, by earth, or some other

material, brought from a distance. The lot which runs from Greenwich-street to the water's edge, south of Rector-street, on which all the jakes in question were located, was formerly occupied for many years, as a stone-cutter's yard and shop, and was altogether filled up with the chippings of marble and sandstone, in pieces of every dimension, from several pounds weight, down to the smallest grain of sand. Earth, as I shall call it, for want of a more appropriate name, thus made, is hard and very porous, and freely admits the flowing tide-water through it in every direction.

The first pit made for a new sink, was directly in the rear of, and a few feet from, Mr. Bakewell's duck store. It was at first contemplated to make this pit ten feet deep; but, from the inconvenience experienced and expected, from the free ingress of the tide, it was carried to no greater depth than seven feet. The earth taken from it, of the nature above described, had no offensive smell or appearance, and was thrown into Washington-street, where some hollows required filling up. The next pit made for a new sink, was thirty feet east of the one above described, in the same kind of earth, and the same difficulties were experienced from the ingress of the tide; but the circumstance which gave the labourers the most trouble, and the sink a bad name, as causing fevers, was in consequence of the spot chosen for the pit, being so near to one which was in daily use, on an adjoining lot, that when the workmen had descended to the depth of five feet, a part of the thin partition of earth between the old sink and the new pit gave way; thus permitting a small portion of the contents of the former to escape into the latter. This was necessarily removed with the earth, to the annoyance of some individuals in the neighbourhood. It was not, however, suffered, as some have stated, to lie in the street, but was carted to the battery, to fill up a dock in that vicinity. The number of persons employed, from the middle to the last of July, about this affair, were from twelve to twenty, not one of whom was attacked with fever.

The third nuisance, mentioned by some as a cause of disease at this place, was the stagnant water which remained in the

gutters until it became offensive. Rector-street, however, from its descent, will not permit water to stand in any part of it in greater depth than two inches.

The above enumeration includes every thing which has been spoken of as a cause of the fever on the spot where it first appeared. Some ingenious persons did, however, labour hard to prove that the source of the mischief was to be found in the burying ground attached to Trinity church. In order to confute every argument which they have advanced in support of their opinion, it is only necessary to state the fact, that the poison which actually did cause the disease, was spreading daily in every direction, from the place of its first appearance, for one month before it reached the vicinity of that church-yard.

The second grand division of causes which were likely to originate the late fever in Rector-street, comes next to be examined. These, all of foreign origin, are to be sought after in suspicious vessels, which were permitted to come to the wharves in that vicinity, or in goods landed there from vessels known to be infected. The officers of the quarantine establishment for this city, have designated, as four-day-vessels, a certain class, which are always to be regarded as suspicious, because, owing to a defect in the law, they are privileged to carry pestilence wherever they go; thus jeopardizing the lives of thousands, and rendering all other quarantine regulations useless. To prove the above allegation, it is only necessary to state a few facts. First, it is well known, that the different Boards of Health of the cities and towns on our seaboard, are, five times out of six, among the last public bodies, which come to the knowledge of pestilence in their respective districts. Hence, in almost every case where a pestilential fever appears in any city, from one to two months elapse before its Board of Health become officially informed of it. During this period the public authorities of the place continue to issue clean bills of health to every vessel sailing from it. One of these vessels, though infected with the poison of yellow fever, may arrive at this port with a healthy crew, or one or two of

her hands may have sickened or died on her passage, and the officers, as it is always their interest so to do, may disguise or conceal the real character of the disease. This vessel, on her arrival here, is liable to be stopped only four days at the quarantine ground, to undergo the too often useless operation of white-washing. She is then permitted, by law, to come to our wharves, with a strong probability in favour of her kindling pestilence wherever she touches. Cases of the above description are by no means of rare occurrence; it is, however, unnecessary to mention more than one or two. The ship Asia sailed from New-Orleans some time in the month of August last, and arrived here on the 18th of September. She left three of her crew sick with yellow fever, in the hospital of that city; yet, strange as it may appear, she brought a clean bill of health, signed by the proper authorities of the place. And, during the first three weeks of the late fever in this city, it is well known that vessels sailed daily from this port with clean bills of health: one of these, the ship Illinois, however, which left Rector-street wharf, on the 16th of July, did lose a passenger by yellow fever.

Again, a vessel may cruise between the different ports in the West Indies for months, and at last venture near enough to some sickly place to receive the poison of pestilence on board, which may, or may not, affect her crew, on her passage to the north. She may touch at Charleston, or some where else in the southern states. When she arrives here, she reports herself healthy from the last place she was at, within the limits of the United States; she is then, by law, detained four days at the quarantine ground, for purification, from whence she is permitted to come to the city to discharge her cargo, with a possibility of poisoning whoever approaches her.

To the four-day class belong all the suspicious vessels, lying near Rector-street wharf, at the commencement of the late fever. These were five or six in number; first, the schooner Florida, Captain Johnson, of which much has been said. This schooner was employed as a cartel, in 1821, to transport

to the Havana, at the expense of the Spanish government, such citizens of St. Augustine, as chose to leave that place upon its cession to the United States. She arrived at Havana in the month of August, crowded with passengers, at the time when that place was extremely sickly with yellow fever, which was soon introduced on board, affecting a number of persons, several of whom died, and her commander had the disease. In September, she returned to St. Augustine, where it is stated, that Captain Johnson sent a quantity of clothing on shore, which was washed by a woman, who had a husband and three children, all of whom, five in number, were poisoned by the clothes, and all died with yellow fever; the first victims of that disease on shore for a number of years. The poison, now located at the house of the washerwoman, spread from thence in every direction over the town, attended by its usual accompaniments, death and desolation. The Florida, some time after this, sailed for this port, and arrived here in November: she sailed again for Cape Florida on the 26th of the same month.

Here a question arises, whether a few days exposure to our November temperature was sufficient to destroy the infection, of which no doubt she contained much? She spent the last winter in the West India seas, part of the time near Cape Florida, but occasionally looking into the Havana: in the spring she became very leaky, and about the first of June sailed for this port for the purpose of repairing: after stopping at the quarantine ground four days, she arrived at a wharf near Rector-street: on the 21st of June her little cargo, consisting of old iron, old junk, snuff in bottles, &c. was taken out and placed on the wharf, and two large turtles which she brought, were made fast with ropes, and put into the water alongside of her. She was then hove down, her bottom taken out and a new one put in. In her repairs, from six to twelve men were employed for three weeks, none of whom were taken ill, except her blacksmith and his apprentice, who, according to their own statement, were both on board her, and both among the first affected with the fever. Besides this blacksmith and

his apprentice, five others, four of whom are since dead, are known to have visited this vessel, or the dock at which she lay, viz. John Reder, Andrew Thomas, Leonard Archer, William Kimball, and William Morgan. Now I have not told this long story about the schooner Florida to prove, or even to infer, that she was an infected vessel when she arrived, but on the contrary, I think with our able health officer, that she was not. She however, unintentionally I think, did some mischief; for by placing her turtles, one of which was so large as to attract much notice, alongside, she drew many, especially children, who were inclined to see those animals, into the very centre of yellow fever poison, which was then discharging from another source to be noticed hereafter.

The next vessel to be mentioned, is the ship Shamrock, from Matanzas, which arrived on the 11th of June: she was quarantined four days, and then permitted to come to the wharf near Rector-street, although it was soon discovered that she had sailed from a sickly port. We have however reason to believe she fortunately escaped infection; so with the Elias Berger and schooner Nile; they were both from sickly ports, though believed to be otherwise at the time of their arrival, and both quarantined four days. They did not, however, either of them, arrive early enough in the season, whether infected or not, to have caused the yellow fever. The last vessels I have named, with their attending circumstances, principally to show the absolute necessity of putting the four-day vessels under very different regulations, if the health of the city is to be preserved by quarantine establishments.

The part of my subject next to be considered demands the most serious attention from every inhabitant of this city, whose wishes and exertions are united for the attainment of that invaluable blessing, the public health; as it shows, how absolutely the appearance of that particular kind of pestilence, called yellow fever, is enchained to an intercourse with known and admitted foreign infection. Indeed, it may be truly affirmed, that the substance does not more certainly regulate the movements of the shadow, than does the intercourse here

mentioned, the appearance of that fever in this city and its neighbourhood; and here I beg leave to state a curious fact, but whether one to be laughed at, or treated more gravely, I will not now decide, viz. that the sticklers for the local origin of this disease have never yet found a nuisance which was, in their opinion, sufficient to produce it, but it happened some how or other, to be always located in the very centre of this intercourse with foreign infection. We find, that during the last twenty years, without going further south than Perth Amboy, the yellow fever has been communicated to this city, or to some town or place in it vicinity, in more than twenty instances; and in only two or three instances out of these more than twenty, has there been the least difficulty experienced in pointing with certainty to the vessel which brought it, or to the intercourse through which it was communicated; and in the two or three instances, where the poison has not been fairly traced to the vessel which brought it, it has been owing to the want of proper inquiry timely made on the subject, or to difficulties inseparable from such inquiries: for the persons who know most about the particular facts, are frequently much disposed, for obvious reasons, to hide or at least discolour them. And where were these two or three instances of pestilence found, in which some difficulty was experienced in tracing them to their proper sources of infection? Not at the Wallabout, nor at Brooklyn, nor at Perth Amboy, nor at the Quarantine Ground, nor at Middletown, nor New Haven, nor New London. No, it was at none of these small towns, whose most minute intercourse is continually exposed to the public eye; some of which have, however, suffered severely several times by the introduction of yellow fever. It was in New-York that the difficulty was found, a city, including sojourners, containing a population of 140,000 souls, and probably enjoying a larger commerce with sickly places than any other in the universe of the same number of inhabitants. A city, not like many in Europe, surrounded by a "Corps Sanitaire," armed with bayonets to prevent the introduction of foreign pestilence, but depending for protection in that respect on a

quarantine establishment, which is regulated by laws so unaccountably formed, as not only to admit daily to its wharves, vessels as likely to be infected as otherwise, but also to admit to any part of it, during the greatest heat of summer, a cargo known to be infected, or even three or four such cargoes at a time. And, as it is now admitted, because proved beyond the power of contradiction, that a sailor's bed, or even his bundle of clothes, may convey enough of the subtle and invisible poison which causes yellow fever, from a sickly place in a distant country to a healthy family or community in this, to originate that disease with all its horrors; it is to me really a matter of astonishment, that hundreds of instances have not occurred, which could not be traced at all, instead of two or three, which have presented difficulties on that account.

With the foregoing general observations touching the subject under consideration, I have thought it best to preface the particular instance of intercourse with admitted infection, now to be related. It seems our merchants claim under the colour of law, or long usage, the privilege to bring to this city in lighters the cargoes of all vessels at the quarantine ground, which may have arrived there from sickly ports, and consequently subject to a detention of thirty days, regardless of any disease or mortality that may have occurred on board them. Of this description were the four following vessels: viz. first, the Spanish Soldier, which had sixteen days passage from the Havana, and arrived on the 25th of June: one man was sent on shore sick of yellow fever before she sailed, who died on the 30th of May; another sickened of the same disease, and died on the 17th of June during her passage to this port. Second and third, the brigs Ambuscade and Abeona; the former of these vessels had thirteen days passage, the latter fourteen; they were navigated by acclimated crews, composed chiefly of Spaniards, who of course were not subject to yellow fever: on their arrival here they reported the Havana as very sickly. Fourth, the ship Eliza Jane; she had thirteen days passage, and arrived here on the 1st of July: two of her crew died of yellow fever, one on the 10th of June, just before she sailed,

the other on the 18th of the same month, while on her passage. The cargoes of these four vessels, which I have named above and described, as known and admitted by all parties to be infected, for the reasons above mentioned, began to come to the city on or about the 28th of June; and between that day and the 6th of July, nearly two thousand boxes of Havana sugar had been landed within a space of two hundred and forty yards, of which space the bottom of Rector-street appears to be the centre. On the 10th of July, five days after the principal part of the infected goods, viz. twenty four lighter loads had been landed, the first cases of the fever appeared. The merchandise, to which we charge the introduction of poison in this instance, was wood, made into boxes containing sugar, and therefore judged by many to be incapable of imbibing infection. This opinion, in connexion with some others, which seem to have influenced the legislature at the time of their passing the laws establishing quarantine regulations, I wish to examine, because I believe them to be not only very erroneous in themselves. but very dangerous to the interests of this city, when made the basis of practice.

The opinion above alluded to, held forth by the law and by the merchants on this subject, appears to be founded on the belief, that there has been conferred on man, through the operation of his organs of sense, a capability of distinguishing so nicely in this most difficult and dangerous of all cases, as to enable him to point out with certainty the several articles of an infected cargo, which have imbibed the least or the greatest quantity of the poison which causes pestilence. Now, as it is well known that no such power of judging aright has been gratuitously given to man, and that he only becomes informed of the existence of the cause from its dreadful effects; and as the executors of the law, who in conjunction with others of the same opinion, under the influence of its erroneous doctrines, have made a few experiments to which, though originating from the best intentions, thousands of their fellow-creatures have fallen victims; and as it is certain that the members of the legislature, in common with the rest of man-

kind, have been at all times ignorant of the nature, composition, and chemical affinities of the poison of pestilence; and as it is moreover equally certain that the few facts, which have been slowly developed and arranged mostly in this country, during the last twenty-five years, and which serve to throw a faint gleam of light on some of the laws which seem to govern the causes of pestilence, have not been called in to the aid of those whose duty it is to guard us against its approach, either in the correction of their opinions, or the improvement of their practice; is it to be expected, I ask, under all these circumstances, that the result should have been more favourable than it really has been? or that the best laws should not have been devised and passed for the protection of the public health; and that the practice under these laws should not have been the safest which could have been adopted? All, or indeed the greatest good that could have been expected from legislative bodies, surrounded as they always have been with darkness and difficulties, would have been the passing of a law enforcing yearly the total prohibition of intercourse between this city and infected vessels and their cargoes from May until November: and whenever our experience, founded on facts, can convince us that particular articles of merchandise may be safely exempted from quarantine vexations, then, and not until then, will it be time for enlightened legislators to step in and pass laws in favour of the exemption of particular articles.

The few facts hinted at above, from which I have thought some conclusions of practical utility might be drawn, I beg leave to state as follows, viz.

First.—Great heat, with drought so long continued as nearly to suspend the watery exhalations from the earth, check, in the same proportion, the progress of yellow fever.

Second.—The greater safety in visiting an infected spot in the time of night, after the aqueous vapours have fallen to the earth, than in the time of day when they are rising from it.

Third.—The difficulty experienced in all attempts to cleanse an infected vessel, either by washing, white-washing, ventilation, or fumigation.

Fourth.—The tenacity with which infection adheres to any spot of earth where it once becomes located.

Fifth.—Very heavy or long continued rains, by thoroughly washing the earth's surface, appear to dispose of or carry away so much of the poison of pestilence, as sometimes to almost suspend its progress. This effect was observed at the Old Slip in 1819, from a very copious fall of rain which occurred in October, and also during the late fever from a like storm, which happened on the 20th of September.

Sixth.—The moment any considerable difference of temperature takes place between the earth's surface and the stratum of air in immediate contact with it, though freezing does not follow, that is, when the air is cooled fifteen or twenty degrees below the temperature of the earth, the cause of yellow fever becomes very much weakened, and sometimes altogether checked.

Seventh.—Whenever the temperature of the atmosphere is so low that water is changed to ice, pestilence is invariably stayed.

The above facts have been so uniformly developed in the economy of that kind of pestilence, known by the name of yellow fever, that they may be considered as well authenticated, and from them the following conclusions appear to me to be naturally deducible:

First.—That the poison which causes this disease, be it what it may, unites very readily with water, and in that state of union penetrates freely every substance which absorbs that fluid.

Second.—That this poison cannot act noxiously on the human frame, but through the medium or agency of water; nor even through that medium or agency unless that fluid shall have passed into the state of vapour.

Third.—When aqueous vapours cease to arise from an infected spot of earth, or infected goods, in consequence of extreme dryness, or any other cause, the only operation by which the poison of this pestilence can be applied to man also ceases, and disease is invariably stayed, or, at least, the danger of con-

tracting it greatly diminished, according to our first and second facts.

Fourth.—All articles capable of absorbing water, and which have taken up that fluid in union with infection, are to be regarded as dangerous to the public health, in proportion to the quantity imbibed; and the time of this danger will be more or less protracted, as they dry more or less slowly. And the wood of which ships are composed, and that found among the cargoes of infected vessels, become highly imbued with this poison, for this article absorbs water like a sponge; and in consequence of drying more slowly, it continues to throw out infection for a longer time than most other materials. This is the reason why a ship may be so cleansed by washing and ventilation to-day, as to be visited with comparative safety; but in the act of drying next week, she will throw out so much poison as to become re-infected herself, and to infect the atmosphere in her immediate neighbourhood; and thus, also, the water which has become infected, and has sunk two or three inches into the ground will remain there, undisturbed by the wind, until the action of a warm sun shall bring it forth in a state of vapour, to do the work of mischief and death, according to our third and fourth facts.

If the conclusions here are fairly drawn and warranted by the facts, the sugar boxes, which were landed by the thousand at the bottom of Rector-street, in the beginning of last July, were unquestionably the most mischievous articles which could have been brought into this city. If very large quantities of pure wholesome water be suddenly thrown upon infected spots of the earth, the poison of fever is carried to so great a depth below the surface, or so washed away, as to be rendered nearly harmless, according to our fifth fact. Here I beg leave to suggest the propriety of introducing into this city a supply of good water, sufficiently large to wash freely and frequently over any part of it that may become unhealthy.

During autumn, as the influence of the sun becomes diminished, and the cold northerly winds begin to prevail, the atmosphere is cooled much more rapidly than the surface of the

earth; hence the watery exhalations from it will be immediately condensed, and returned to it without their having obtained sufficient elevation to become very mischievous, according to our sixth fact. The condition of water is so completely changed by being frozen, that all evaporation from it of the least importance ceases; hence it is no longer the medium for the circulation of the poison of pestilence, and the poison of pestilence also ceases to circulate for want of such a medium, and the watery vapours afloat in the atmosphere, cease to be united with pestilential miasm, in consequence of their precipitation to the earth in the form of hoar frost; thus purifying the air, and staying pestilence, according to our seventh fact.

If what I have stated for facts, are really such, and I have no doubt they are, and if the conclusions which I have drawn from thence are just, as I believe them to be, the first step may then be said to be made towards the establishment of something like rational quarantine regulations; which I do not think exist, at present, in any part of the commercial world: for while our own are so loose as to permit infected vessels and cargoes to pass them by wholesale, those of Europe are so rigid as to quarantine a vessel for forty days in one of their out-harbours, although she may have arrived in the middle of winter, so overloaded with ice, as to be in danger of sinking from it. For this purpose it is only necessary to make these practical distinctions between the several articles of merchandise which may come into our ports, on board of infected vessels, and to divide them into three classes: First, such goods as do not imbibe water at all: to this class belong all metals, and articles formed of glass, marble, &c. These, to be cleansed, need only to be well washed and thoroughly dried. No washing and drying, however, which have been done on board of infected vessels, or in infected districts of a city, are to be relied on with perfect safety. Second, such goods as absorb water freely, and part with it quickly: to this division belong clothing and bedding, and, if not too closely packed, even cotton and sponge; repeated washing and dying

will, no doubt, cleanse these articles. Third, such goods as take up water freely, and part with it very slowly; for instance coffee and hides, if large quantities of them be together. At the head, however, of articles of this class, always charged for the work of mischief, will be found infected vessels themselves, so long as they continue to be made of wood, and the portions of their cargoes, which are composed of that material. In cleansing these portions, the exercise of that great virtue, long patience, seems to be absolutely necessary, as the poison they have imbibed lies out of the reach of ordinary washing, fumigation, or ventilation. Certain agents may be found, from their avidity for water, possessing great exsiccant power; among which are quicklime, and some others. These may, therefore, be sometimes used with advantage to intercept the matter of poison, or to render it inert, by depriving it of its medium for circulation; but they ought always to be employed in a state of extreme dryness, and not saturated with water, as in white washing. Indeed the lime, which was spread over a part of the infected district, during the late fever, might have done some good, if those who placed it there, had not taken effectual measures to render it *useless by slaking it at the time of laying it on.* And as we know that any thing placed alongside an infected vessel, is liable to become itself infected; and as we also do not know how small a quantity of this infection, when introduced into our city, may originate a most desolating pestilence, I cannot avoid thinking that it would be our wisest course to run no risks on this head. I therefore beg leave to suggest the propriety of preventing those vessels called "lighters," from approaching the wharves for at least some days, after having lain for hours in contact with vessels known to be poisoned, whether they have received goods from them or not: neither would I permit goods of any description to be brought into the city in the same lighters which conveyed them from the vessels to the place of their purification.

The detention of persons who may arrive at the quarantine-ground, on board infected vessels, may be with perfect safety reduced under plain and easily practicable regulations

to one fourth of the time now prescribed by law. The adoption of a reasonable practice in this particular, would go far towards removing quarantine vexations.

The following speculations and opinions, interspersed with facts, are thrown out merely to invite the attention of the curious to this subject.—By the time the poison, which causes yellow fever has passed fifty yards, more or less, from the source whence it is emitted, it has become too much weakened by diffusion to excite that disease, and no length of time, the source continuing of the same extent, can enable it to act hurtfully to a greater distance; because in an ever-changing atmosphere, it is constantly and rapidly passing to the point of diffusion. Fifty yards I will suppose to be the average distance to which it can act with effect in this country, from its source of emission, varied however a little by different states of the atmosphere. In some tropical climates, however, this poison appears to be carried with noxious powers to a much greater distance, and the following seem to be the reasons why this happens. In some of these countries in which the poison is produced in great quantities, or found to abound, a profuse watery exhalation arises from the surface, elevating with it into the atmosphere much of this poison, which, owing to the considerable difference of temperature, between the day and the night, a large quantity of this vapour is rapidly condensed, and returned towards the earth in the form of a West Indian dew, which has been generally remarked to produce the yellow fever. The moment this condensed vapour begins to descend, what is called the land-breeze sets in, which always blows from the land to the water, no doubt carrying with it much of this infected dew; hence, the danger of vessels approaching within a mile or two of the shores of these countries, if sickly, especially during the night.

But yellow fever does extend its ravages, gradually and slowly over large cities; its source then by some means or other, must become proportionally extended: now it is evident, for the reasons given above, that this extension cannot be effected in the atmosphere above the earth's surface; we

must therefore look for some place where it can be effected, and this place we find at the surface of the earth. The humidity in that situation seizes the poison with the greatest avidity, either to be carried into the atmosphere by evaporation from the production of disease, or to be washed away out of mischievous circulation, if the spot, where it becomes located, should be inundated with water. If, at the time of the location of this poison on a spot of earth, heat and moisture are properly combined, so as to favour its operation, the extension of its district begins, I apprehend, in the following way: a portion of the poisoned vapour, which arises from an inch of infected surface, falls on the adjoining inch of uninfected surface, and by it enables that, in its turn, to poison another, and so on; thus slowly enlarging the infected part of a city, until pestilence spreads far and wide.

Of the chemical nature, or composition of the poison of which we have been treating, nothing appears to be known. We have, however, reasonable grounds for believing that it is not of animal origin, and therefore does not excite a disease personally contagious. It is not of animal origin; for, in countries where it most abounds, all the inhabitants being acclimated, the fewest persons have it: it is there the stranger's disease; and all the facts, experiments I will call them, which have been quoted to prove yellow fever personally contagious, have been too loosely made. They do touch this question, but they prove something else, which is, that this fever is personally infectious; that is, the poison which excites it may be carried in any articles of clothing on or about a first person, so as to communicate disease to a second, whether the first may have the disease or not. This is personal infection, as I understand these words.

I said above, that this infection did not act hurtfully more than fifty yards from its source of emission; but after it has passed these fifty yards, and become too much diffused to act noxiously, does it cease to act at all? I think not; for I believe it to be a stimulant of the highest exciting powers when applied to the human body, and that, like all other poisons,

when the dose becomes too small to kill, it cures. Suppose we administer to a weak man too large a dose of opium or arsenic, what is the consequence? Why, he is stimulated into painful sensations, disease, and death: but if we administer to the same weak man, small doses of either of these active poisons, he is stimulated into pleasurable sensations, strength, and health.

From this view of the poison which excites yellow fever, I am led to account for a fact, which occurs so universally as to be generally noticed, which is, that when that disease is spreading, for a considerable distance in advance, of the infected district, all other diseases, especially those of debility, seem to be banished or suspended. This suspension of such diseases, I think may be ascribed to the medicinal effects of this stimulant poison, which is diffused through the atmosphere in quantities too small to cause disease.

Yellow Fever in Cheapside and Lombardy Streets.

When one half of a large city is so overspread with pestilence, that thousands of its inhabitants have been forced to fly to its more healthy parts for safety; and when the intercourse of necessity, of business, of friendship, of curiosity, and of knavery, have each had their share of influence; it appears very idle to expect to trace every case which may occur elsewhere, to the district first infected: each kind of intercourse here mentioned, occasionally, no doubt, has its victims. When, however, the communication has been carried on for the purposes of knavery, it is not very likely that those who have sickened from it, will be very communicative on the subject, unless the facts be wrung from them at the hour of death by the pangs of a guilty conscience. The particular cases of fever now to be noticed, were caused by the intercourse of friendship or affection: and, as we should expect in all like cases, we find in this, that the family who suffered most, held more communication with admitted infec-

tion, or with the infected district, than all the people of the Seventh Ward beside. The facts are these: on or about the 7th of August, Mr. Samuel Ward, who resided in Lumber-street, within the infected part of the city, sickened with yellow fever, and was reported as such, by Doctor Perkins, to the Board of Health. Here he lay ten days very ill. During this time, his father's family, who lived at No. 20 Cheapside-street, manifested on this occasion the most laudable anxiety for his welfare. He was not only visited daily, or oftener, by his father and mother, but his brother, a dumb boy, fifteen years of age, and a sister of thirteen, were constantly with him, as nurses or assistants. They slept at his house every night until the 17th, when it was thought that he had so far recovered as to enable him safely to leave Lumber-street, which he did, and went to his father's house, from whence he set out for the country next day. He brought with him from Lumber-street a quantity of clothing, which, however, was said to have been washed there; that is, washed in pestilence, and dried in the poison of pestilence. About five days after he was gone, his brother and sister above-mentioned, who had returned with him to their father's house, both sickened with fever. The sister's case was not very severe; but the brother was extremely ill, and as he could not speak, he expressed his sense of pain by laying his hand on his head, his back, and stomach. On the fourth or fifth day of his disease, he was taken with profuse bleeding from the nose: after this, they both gradually recovered. Now, there is not in my mind any manner of doubt, but that both these children had yellow fever. A few days after this, another daughter of the elder Mr. Ward was taken ill. Doctor Boyd was called in, who, no doubt very correctly, reported it yellow fever: and in a few days more, the elder Mr. Ward and another son were seized with the same fever, both of whom died. Mr. Ward's house, and its immediate vicinity, may now be fairly considered a second infected district, located only about sixty or seventy feet from No. 4 Lombardy-street; where, it appears from the following affidavit and certificates, that another spark of the poison was set afloat:

No. I. (COPY.)

Jonathan Darrow, cartman, Number 5 Jefferson-street, in the city of New-York, being duly sworn, deposeth and saith:

That on or about the second day of September, in the present year, he was requested by a young man, at or near the New-market, at the bottom of Catharine-street, in the city aforesaid, to take up and put on his cart, two chests, a bed, a hammock, and a bundle of clothes, tied up in a blanket. This young man had with him a sailor, as from his dress he appeared to be, who appeared to own a part of the things aforesaid. The young man first mentioned, after the articles were put on the cart, got on himself with them, he then requested this deponent to drive to number four Lombardy-street, which request was complied with. While on the way thither, a conversation took place between this deponent and the said young man, in which the latter observed, "a part of these things now on the cart does not belong to me, but they did belong to a young man who lately died of yellow fever," or words to that effect. And this deponent further says, that he drove his cart to the house, number four in Lombardy-street, where he delivered that part of the goods, to wit, one chest, one bed, and the blanket containing the clothes, which the young man above mentioned had charge of, to an old woman and a young woman, who both received the goods, and placed them in the entry of the house number four Lombardy-street. And this deponent further saith, that he then drove into Bancker-street, about half-way between Catharine and Market streets, where he delivered the remaining part of the goods, to wit, one chest and a hammock. And further this deponent saith not.

(Signed) JONATHAN DARROW.

Sworn before me, this 18th day of December, 1822.

(Signed) HARRIS SCOVELL,
Assistant Justice.

No. II. (COPY.)

We, whose names are hereunto subscribed, hereby certify, that we saw, during the first week of September, in the present year, several different times, a chest on which was placed a bundle of clothes, or bedding, as they appeared to be, tied up in a blanket, as we believe, standing in the entry of the house number four in Lombardy-street.

Given under our hands, the 18th day of December, 1822.

(Signed) WILLIAM H. MOTT,
FRANCIS SCHRODER,
ELEBRED POLHAMUS.

No. III. (COPY.)

I, Sarah Potter, residing at Number twenty-three Lombardy-street, hereby certify,

That I was employed by the late Mistress Cary and the late Mistress Snow, on Tuesday, the third day of September, in the present year, to wash some bedding and clothes, which I was informed had, or did belong to some person who had been sick, and that I accordingly did wash one rose blanket, one bed quilt, one pair of socks, one handkerchief, one waistcoat, and three shirts, and that the shirts were very offensive and very yellow. And that, while I was washing the said clothes, the late George Washington Mott came near to the washing-tub, and inquired of me whether I took in washing.

Given under my hand, this 8th day of December, 1822.

(Signed) SARAH POTTER.

On two occasions, it seems that infected clothes or bedding may be peculiarly mischievous, viz. when they are first opened in a warm atmosphere, after having been long closely packed up; and again, when they are put into warm water, for the purpose of washing. The profuse vapour of warm water seizes the matter of infection with the utmost avidity, and conveys it, in its most active state, to the noses, mouths, and lungs of the bystanders. This fact was shown in a very striking manner; for, at the time the clothes referred to in document No. 3, were washing, George Washington Mott, having that day come to the city, was, with Mrs. Cary and Mrs. Snow, standing by the tub: they all sickened with yellow fever on the same day, and all died within a few hours of each other. The infection in Lombardy and Cheapside streets reached to Catherine-street; thus taking possession of three principal avenues which lead to the wharves and the New Market, from the upper part of the city: and to this second infected district, no doubt, we may justly charge some cases of fever which occurred to the eastward and northward of it, and which were not at the time fairly traced: a case in point was that of Mr. D. Rogers. There were, however, three or four cases that fell under my own observation, which were reported to the

Board of Health as yellow fever, in the upper part of the city; aboat which I shall not speak, unless I was disposed to trace a drunken frolic, or something as unlike yellow fever as drunkenness, to the infected district.

I shall conclude with the case of yellow fever that follows, and which was among the most mischievous that occurred in the upper part of the city, during the late autumn, causing more alarm, and consequently more unnecessary removals than any other. This was owing to two reasons; first the supposition of it being impossible to trace it to the infected district; second, it being ascribed to a broken sink in the neighbourhood, a thing which too often happens in like cases. The subject of the case here alluded to, was Mr. Scott, who resided at 122 Bancker-street, by trade a tailor. He had received his work for months by the job from Messrs. Pierson and Jacobs, whose store is at the corner of Maiden-lane and Nassau-street. For wages or work he went there two or three times a-week during August, and until the 10th of September, on which day the store was closed on account of the alarming progress of the fever in the neighbourhood, as several cases had already occurred at the noted sugar-house a few yards distant, which terminated fatally. On this day, the 10th of September, he settled with his employers, but was detained there much longer than usual, owing to some difficulty in his accounts. After receiving his wages, as he stated, on his return home he went through Maiden-lane to Broadway, merely to witness the desolation of that part of the city. Six days after this, to wit; on the 16th of the same month he sickened of yellow fever, and died on the 23d. Another journeyman employed by the same firm, who was at the store on the same day, sickened about the same time that Scott did, and was reported to the Board as a case of yellow fever, but recovered. Mr. Pierson, one of the partners of that firm, however, soon after this took the disease, and it proved fatal.

D. D. WALTERS.

REVIEW.

A TREATISE *on the use of the* PRUSSIATE OF IRON, *or Prussian Blue, in Intermitting and Remitting Fevers.* By WILLIAM ZOLLICKOFFER, M. D. &c. Frederick, Maryland, 1822. pp. 32.

IT has been very much the fashion with a certain class of physicians, to oppose the introduction of any new medicines into our materia medica, upon the ground that the list of our remedies is already unnecessarily extended for all the practical purposes of our profession; that the evils attending an extensive pharmacopœia are numerous and dangerous; "that it renders us incapable of duly estimating the real value and medicinal properties of each remedy in particular;" "that if the truly efficient, but small number of articles of the *armoury of medicine*, are incapable of doing all we wish, it is scarcely to be expected that an extension of their number will add to our means of cure. It is not number, but quality we want, for the few indications which our profession points out to us." In consistence with these views, it is triumphantly asked, "among the *cathartics* can we possibly be deficient, if we limit the number to about a dozen, viz. jalap, rhubarb, senna, manna, castor oil, salts, cream of tartar, sulphur, magnesia, aloes, and, above all, calomel?" "For *emetics*, can we possibly require more than ipecacuanha and emetic tartar, together with two or three others of inferior powers?" and so on of the other classes of medicines. "If, with all these," it is added, "scarcely exceeding half a hundred, we cannot accomplish our intentions, it is much to be feared, success would not follow from a greater augmentation."

These, and sundry other speculations, equally sage and philosophical, were, not long since, formally set forth in a sort of manifesto,* by one of the professors of a neighbouring university. Now although we do not hesitate to admit that our pharmacopœia is susceptible of considerable simplification, yet sorry indeed would we be to rest contented with so barren a list of remedies as that proposed by the learned professor. We will venture to assert, without fear of contradiction, that unless the physician practises according to the most rigid Brunonian principles, he will very frequently find himself at a loss with "the armoury of medicine" here supplied.

Our limits will not permit us to enter upon the many and striking proofs which might be advanced to establish this position. We cannot, however, leave this subject without remarking, that throughout the whole of his reasoning, the writer we have alluded to, goes upon two principles, which are wholly gratuitous. In the first place, he takes it for granted, that our materia medica is already perfect, at least so far as any *additions* to it are concerned. That we are, at present, in possession of all the medicinal agents that can be necessary in the cure of disease, and that the only improvement we are called upon to make is that of retrenchment. This is indeed a bold assumption, when it is recollected that the wisest and greatest men have confessed and regretted the imperfection of medicine. In the second place. he takes it for granted, that the only distinguishing properties of medicines are those which characterize the artificial and arbitrary classes under which they are ranged. For instance, a medicine is either emetic, cathartic, diaphoretic, &c. and if we have one or two powerful articles in each class, we want no more to enable us to fulfil every necessary indication. This is certainly a most limited and untenable view of the operation of medicines. So far

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*Cursory Remarks in relation to the objects of the Materia Medica and Pharmacy. By John Redman Coxe, M. D. Philadelphia Journal of the Medical and Physical Sciences, vol. i. p. 34.

from this being correct, scarcely any two articles, even in the same class, will be found to possess similar properties. Every purgative is not equally proper, where catharsis is indicated, nor every emetic equally salutary, where vomiting is called for; on the contrary, one article may be decidedly injurious, while another, of the very same class, may be eminently beneficial. From this it follows, that so far from restricting ourselves to our present knowledge of medicinal agents, we should be stimulated to the discovery of those hidden treasures, which, we doubt not, are still reserved for the benefit of suffering humanity.

As an illustration of the advantages to be derived from an extensive pharmacopœia, we do not know that we can select any thing more striking than the class of tonics, and especially in their application to the cure of intermittent fever. It must have occurred to every practitioner of any experience, to have met not unfrequently with cases of this species of fever, in which the ordinary tonics completely failed to arrest the progress of the disease, which nevertheless yielded to others even of a simpler character and of milder virtues. Peruvian bark, for example, has failed where serpentaria and camomile flowers have proved successful. A pointed instance of this kind may be found recorded in Hillary's Observations on Barbadoes, (p. 92.) If then, from some peculiarities in the disease, or in the constitution of the patient, or from any other cause whatever, it is found that one intermittent fever yields to bark, while another, apparently similar in character, requires a different tonic to check its career, it proves most conclusively, not merely the propriety, but the absolute necessity of having a varied and extended list of tonics, to which recourse may be had in cases of emergency, or in the failure of those more commonly in use; and it exposes most decisively that wretched system, which would fritter down the whole materia medica to a number of medicines, "scarcely exceeding half a hundred."

Entertaining such views, we have been very much gratified in finding our list of tonics augmented by the addition of an

article, which, according to the representations of Dr. Zollickoffer. promises to be extremely valuable; we mean the *Prussiate of Iron.*

Our author gives the following account of the manner in which he was first led to its use.

"The circumstance that first led to the discovery of the invaluable virtues of the *Prussiate* of *Iron*, as a remedy of very great importance, and worthy of the particular regard and attention of practising physicians generally, was such as might be supposed scarcely entitled to notice, but not unfrequently, occurrences apparently unworthy of the slightest observer, become of the most interesting and important nature to mankind.

"In the latter part of the summer and fall of 1821, more cases of intermitting and remitting fevers claimed my attention, particularly among children under two years of age, than I recollect seeing, during an extensive practice, since I first entered into the laborious and arduous duties of the profession. And this kind of patients claimed my peculiar sympathy, regard, and attention, in consequence of my being fully aware of the existing difficulty of treating them successfully, should the disease not yield to the free and discretionary use of emetics and cathartics; inasmuch as I was confident that barks could not, in the generality of cases, be given in doses suitably large, to produce its revolutionizing effects upon the system, from the circumstance of the exceedingly unpleasant and peculiar taste accompanying its internal exhibition, which I knew to be a very frequent cause of their being refused by the little sufferers, and if taken into their extremely delicate stomachs, of their producing irritation and causing them to be rejected. And being thus situated for want of a remedy equally prompt and effectual with the cort. peruvi. by which I might, with perfect ease, control the unruly and distressing paroxysms of intermitting and remitting fevers; and supposing that nature was not so limited in her gifts as to have provided but one remedy, and that from the vegetable kingdom, for the removal of a disease so general as the one under present consideration, I concluded, after reflecting upon a number of articles that

might be supposed, though they had never been recommended, to be productive of some practical utility, to give the Prussiate of Iron a fair trial. I had at this time seven cases in which I exhibited this remedy, and the result of which exceeded my most sanguine expectations; for five out of the number were relieved, without being subject to an attack of the disease for some time."—pp. 11, 12.

Dr. Z. next proceeds to compare the Prussiate of Iron with the other preparations of that metal, with arsenic and Peruvian bark, and as might naturally be expected from the enthusiasm of a supposed discoverer, gives it the decided preference over all of them.

"The *sulphate* and *carbonate* of *iron* have long since been noticed by most writers on the materia medica. These articles certainly possess virtues, as medical remedies, that render them worthy of attention, and deserving of the place which they have obtained in the list of tonics: though from experience, I can boldly assert, that neither of them can be considered of the least importance, in a practical point of view, when compared with the *Prussiate of Iron*. The two former articles, as tonics, are slow in producing the effects peculiar to this class of remedies, upon the human system, and from this circumstance, the result arising from their administration is seldom visible under two or three days: neither have I found either of them competent to the cure of *intermitting* and *remitting fevers*. The *Prussiate of Iron* in this respect, differs very materially from these preparations, in being more certain, prompt, and effectual, in fortifying the subject of this distressing malady against the return of future *paroxysms*.

"The *oxide* of *arsenic* exhibited in the form of the sol. arsen. Fol. min. is an article that is highly prized by many practitioners, on the account of its supposed superiority as a remedy, over all others, in the treatment of intermittents. The utility of this compound cannot be doubted; although the consequences resulting from its internal exhibition are not unfrequently of such a nature as to render the further use of medical aid essentially necessary, in order to relieve the patient

from a partially diseased state, that has been brought on by giving it in doses rather large, as likewise by continuing its use for too great a length of time, without occasionally suspending it. Another inconvenience occurs, which would appear to be an objection to its use under all circumstances, and that is, of administering it to the more illiterate class of society, many of whom are of the opinion, that if a small quantity of a medicine is calculated to be beneficial, an increased proportion must be productive of the most certain and happy effects: and by this mode of false reasoning, they are induced to give as much more (out of the phial that may be left them) as their judgment may, on the occasion, influence them to conceive necessary, without having any reference to the rule prescribed by the physician for their observation. Every practising physician of experience in the profession must certainly be well acquainted with this circumstance, inasmuch as they have, from time to time, in paying their visits to the class of people alluded to, discovered this disposition.

The *Prussiate* of *Iron*, which is recommended in this disease, cannot be objected to on this account in consequence of its mildness, and indeed, it is perhaps more so than any other remedy of equal power, with which the present improved and considerably enlarged state of the materia medica acquaints us. It is far more certain, and better adapted to the treatment of intermittents, than arsenic under any form: and it possesses an advantage over this article, in any period of the disease, being a suitable time for its administration; though it has never been productive of nausea even in one solitary instance, where the stomach would not receive, without becoming irritated, every other remedy. I have merely to observe in closing this part of the subject, that it is hoped, that those who are in the constant habit of using *arsenic* will, at least, if not continue to use the *Prussiate* of *Iron*, give it a few trials, in order to test the superior and decided advantages that it has been stated to possess over this article."—pp. 13, 14, 15.

Over cinchona he asserts it to have the following advantages, as a remedy in intermitting and remitting fevers:

"First.—It is void of taste, and may therefore be much more readily exhibited than the *cinch. offic.* which, to some, is extremely unpleasant.

"Secondly.—It may be given in every stage of the disease; while the administration of Bark is confined to the *apyrexiæ.*

"Thirdly.—The dose is much smaller, being from four to six grains twice or thrice in twenty-four hours; or at morning, noon, and at night: while bark, to be effectual, must be given in much larger doses.

"Fourthly.—It never disagrees with the stomach, or creates nausea, even in the most irritable state of this *viscus:* while bark is not unfrequently rejected.

"Fifthly.—In its effects as a remedy calculated to prevent the recurrence of future *paroxysms,* it is more certain, prompt, and effectual than the justly celebrated *cort. peruvian.*

"Sixthly and lastly.—A patient treated with this article will recover from the influence of intermitting and remitting fevers, in the generality of cases, in much less time than is usual in those cases in which bark is employed. In making use of the Prussiate of Iron as a remedy in disease, care must be taken to select that which is of a very dark blue, approaching to a black, having a shining coppery fracture, and adhering firmly to the tongue."—pp. 18, 19.

Dr. Z. concludes his account of the Prussiate of Iron, by the detail of several cases illustrating its success in the cure of intermitting and remitting fevers.

We cannot close this notice of Dr. Zollickoffer's book without correcting an error into which he has fallen, (unconsciously we are convinced,) in supposing himself the first person who ever used the Prussiate of Iron as a medicine. He says, this article "is considered by limners, to be a paint of much value, by combining it in certain proportions with other substances; but as an article worthy of regard and attention, *on account of its application as a medicine, calculated to alleviate the sufferings of fallen humanity, induced by disease, it has never been known.*"—p. 7.

And again in his preface:

"That this remedy will at first meet with many opponents to its general introduction, I am well aware of, knowing that *all new discoveries* have, with considerable difficulty at first, overcome the unfavourable opinions respecting their utility, as applicable to disease."

By turning to Lewis's Dictionary of the Materia Medica, 4th edition, by Dr. Aikin, Dr. Z will be convinced that he has long since been anticipated. Under the article Iron it is stated that "some have made trial of the blue precipitate called *Prussian blue*, and report that it seemed to act as a diaphoretic, and in some cases as an aperient.* Of all the known preparations of iron," it is added, "this promises the least activity: the perfect calces, almost if not wholly inert, are soluble in certain acids, particularly in the marine, but the Prussian blue is not acted upon by any kind of acid menstruum." To the credit of having discovered and established its tonic virtues, we believe Dr. Z. is fully entitled, and the still higher credit of having successfully applied it to the cure of a very common, and not unfrequently, dangerous class of diseases.

Medical and Philosophical Intelligence.

DOMESTIC.

NOTICES *of some of the things received into my Museum and Library, within a short time.* Addressed to the Editors of the New-York Medical and Physical Journal. By SAMUEL L. MITCHILL, M. D. Professor of Materia Medica: Dec. 24, 1822.

FROM the region inhabited by the Caraib and Curaru Indians, toward the sources of the river Oronoko, several hundred miles

* Geoffroy. Memoires de l'Acad. Roy. des Sciences de Paris, pour l'ann. 1743.

above the city of Angostura, arrives, by the kindness of Dr. Forsyth, a quantity of the deadly Curaru or Worraroo poison, prepared in those parts by the natives. There is a quiver full of darts, whose points have been dipped in this terrible venom. The shafts are furnished with cotton instead of feathers, to give them direction. They are conveyed to their mark, whether a bird, a beast, or an enemy, by being blown through a reed or other hollow tube.—There is also a small calabash, containing a considerable quantity of the formidable article, for further use and satisfactory experiments. It is reported to be prepared from certain plants by decoction and extract.

By the generosity of Captain Ridgeley of the U. S. Navy, comes to hand the fœtus of a *cetaceous* animal, taken alive from the uterus of its dam, off Cape Horn. It probably belongs to the *Delphinus rostratus*. This unborn creature is furnished after the manner of land animals with an umbilical cord and umbilical vessels. Young as the individual is, the teats situated near the vulva, one on each side, sufficiently distinguish the sex. From these parts of the organization, in addition to many others, a conclusive judgment may be formed against the vulgar error that a whale is a fish.

The cryptogamic vegetable, from the dreary land newly discovered, beyond Cape Horn, and brought by Captain Napier, turns out, after careful examination by John Torrey, M. D., to be an undescribed species of Roccella. It is in the shape of moss, and in good condition for botanical research. A particular description, with a correct drawing, has been finished by that gentleman, for the purpose of publication. It is a curious subject for reflection that, as the navigators say, the whole upland Flora of New South Shetland consists of a single species.

The *mytilus lithophagus*, or *stone-eating mussel*, presents himself in the cavities of the calcareous rock, which he has penetrated and inhabited, along the shores of the Mediterranean sea, near Genoa. The specimens are in fine order, and present the interesting spectacle of a bivalve mollusca with a delicate shell, perforating the solid limestone, and dwelling in the cavity he forms for himself. By what means he makes the excavation, whether by some mechanical instruments, or some chemical menstruum, is a matter upon which naturalists have not as yet decided.

The skull and lower jaw of the Royal Tiger of Bengal, with the

set of teeth complete, are fine pieces of osteology. The ferocious beast was shot in the neighbourhood of Calcutta by a person upon whom he was about to spring.

I received from Lima the Spanish publication of the distinguished physician, Don Balthazar de Villalobos, on a remarkable epidemic fever which affected various districts of the department of Chancay in Peru, during 1796 and 1797. The book was written pursuant to an order of the governor and the royal court of justice, and is dedicated to the viceroy Ambrosio O'Higgins, and is highly recommended by Don Hipolito Unanue, Professor of Anatomy in the College of St. Fernando, &c. The distemper is reported to belong to the class of acute pyrexiæ, known by the names of *causos* and *putrid fevers*. It sometimes attacked the lungs, pleura, fauces, and brain, and thereby acquired higher malignancy. But the elevated grade of the disorder was produced by dissolving the cohesion, disturbing the equilibrium, and changing the figure of the fluids and humours of the body, thereby destroying their crassis and disorganizing their constitution. This disease, by reason of its violence and mortality, was considered pestilential. It was alleged to have had its origin in Calpa, and thence proceeding a distance of ten leagues, without invading the intermediate population, to have fixed itself in the village of Andalusia, and the neighbouring towns of Huara, Begueta, Mazo, Luriama, and Huacho. The centre of the mortality was almost thirty leagues from Lima. Its *chief victims were the native Indians, the slaves of the agency;* the free persons who suffered were very few. So serious and alarming were the effects, that a consultation, consisting of four eminent physicians, was deputed by public authority to visit the sickly district, and to adopt measures for relieving the sufferers, and of arresting the progress of the malady. Their names were Aguirre, Bueno, Delarocha, and Villalobos ; the last of whom is the writer of the work under consideration. It contains upwards of 150 pages in small 4to. and by reason of the facts and cases it contains, is a very instructive performance.

Another dissection has been made by Dr. W. Anderson and myself of the *Siren lacertina* of the southern states. The individual was received from John P. Thomas, M. D. of Pineville in South Carolina. No doubt can be entertained that this reptile possesses both *gills and lungs*. It seems to be fully established that the spe-

cimens we have had for examination are not larvæ, but perfect animals. There were three principal divisions of the gills on each side of the neck, and three open holes communicating between the external fluid and the throat. The arches have short pectinated processes, like those in fish. They are situated on the under side of the arches, and point toward the throat. Thus ambient air or water seems therefore to pass through and between the arches exactly as in fishes: the fringes on the outside, which have by some observers been considered as the breathing organs, may now be considered as merely opercula or gill-covers; and this opinion is countenanced by our failure to inject their vessels with quicksilver. Notwithstanding this branchial organization, we found within the animal extensive and capacious lungs. These commenced by bronchial tubes from the trachea behind the heart, and extended through the abdomen as far as the cloaca, one on each side of the spine. Upon being inflated, they exhibited a cellular structure. At their origin they were of small capacity; they enlarged toward the middle; they contracted toward their posterior terminations; and ended each in a point very near the vent.

FOREIGN.

Description of an Apparatus for removing Poisons from the Stomach, invented by Mr. Jukes, Surgeon.—We might with justice be considered to be forgetful of the duty we owe the Profession, were we to fail in laying before them a description of a very excellent apparatus which Mr. Jukes, its very ingenious inventor, favoured us with a sight of. It consists of an elastic gum tube, a quarter of an inch in diameter, and two feet and a half in length, terminating at one extremity in a small globe of ivory, with several perforations; the other extremity is adapted, either by screw or by plug (the latter is preferable) to an elastic *bottle* of sufficient size to contain at least a quart of liquid, and having a stop-cock fitted to it, in a similar manner as in the hydrocele bottle. Instead of the *bottle*, a pewter *syringe*, of an equal capacity, may be adapted, in the same manner, to the flexible tube. The operation by

the syringe is performed more quickly, and may therefore, perhaps, be preferred by some. In cases where surgeons have neither *bottle* nor *syringe*, the *tube alone* might be made to answer the purpose, if the operator apply his mouth to the extremity, and thereby institute the office of a siphon.

Application.—The patient ought to be placed on the left side, and the *globulated* end of the tube be then carefully passed to the greater curvature of the stomach, either through the mouth or nostril, as may be thought proper. Having previously filled the *bottle* or *syringe* with warm water, at the temperature of 150°, screw or plug it to the tube, turn the stop-cock, and gently force the contents into the stomach. The then diluted contents are to be immediately withdrawn by pulling up the piston ; or, if the *bottle* be applied, the same effect will ensue from its elasticity enabling it to recover its original form, by which the fluid contents will return, charged with the poison. This operation ought to be repeated, till the water, which is withdrawn, becomes clear and tasteless.

In Mr. Jukes' experiments, first on dogs, and then on himself and others, assisted by Mr. James Scott, Surgeon, in Westminster, the apparatus was proved fully to answer the intended purpose. In these experiments, Mr. Jukes swallowed, first *two drams* of laudanum ; he afterwards gradually increased the quantity, until it reached ten drams : since which, he has administered to several individuals (one of them a female) *one* ounce of laudanum, with an equally successful result. The utility of so well contrived an instrument, arising especially from its being equally adapted to the removal of all the more bulky poisons, must be evident to every one. We consider that Mr. Jukes has rendered an essential service to the profession, and to the community, by its invention.—*Lon. Med. Repos., Oct.*, 1822.

Presence of Urea in the Blood.—M. Ségalas, (with Professor Vauquelin) has lately discovered urea in the blood of dogs which had been deprived of their kidneys ; confirming what MM. Prévost and Dumas, of Geneva, had previously announced.—*Bulletin des Sciences par la Societe Philomatique, &c. Paris. Juin*, 1822.

Test for Magnesia.—In the *Annales de Chimie* for May last, it is stated, that if we spread any clear liquid upon a plate of glass, and

then trace the word *Magnesia* upon the glass so covered, with the end of a glass rod, that word will appear in *white characters* if magnesia be present in the solution ; if not, no such appearance will result. It is, moreover, stated, that this method of discovering the presence of magnesia originated with Dr. Wollaston, and was communicated to the Editors by M. Clement.

In the same journal for July, M. Clement writes as follows to the Editors :

"Gentlemen,—In your number for May last you have given a very inaccurate report of a neat experiment which Dr. Wollaston was so kind as to show me when in London. The precipitation upon the written characters only ensues when the magnesia solution has been previously decomposed by a mixture of phosphate and carbonate of ammonia. This substance, re-dissolved by the excess of carbonate, is precipitated upon the traces of the glass rod, in consequence of the expulsion of carbonic acid by the heat disengaged by friction."—*Brande's Journal of Science, No. 27.*

Detection of Poisons.—A paragraph has appeared in the papers, recommending blue sugar-loaf paper as a test of distinction between oxalic acid and Epsom salt ; it is reddened by the former, but not affected by the latter. This is perfectly true ; but a simpler test consists in wetting the tip of the finger, applying it first to the supposed salt, and then to the tongue—if oxalic acid, it tastes *very* sour ; if Epsom salt, very bitter and saline.

Many restrictions have been suggested upon the sale of arsenic ; the only effectual bar to the mischief that results from it, is *prohibition.* It is of no use in medicine, and should therefore be rejected from the pharmacopœia. Apothecaries and druggists need not then keep it. It is sometimes employed as an instrument of research, in the chemical laboratory, which might be sufficiently supplied through other channels.—*ibid.*

New Blowpipe.—Professor Green has published an account of a new Blowpipe in the *American Journal of Science*, vol. iv. p. 164. The principle, however, is not new ; and an instrument almost exactly similar to it was published by its inventor, John Farey, Esq. *junior*, in the article *Blowpipe*, in the *Edinburgh Encyclopædia*, vol. iii. p. 615.—*Edin. Philosoph. Journal, No. 12*

On the difference between Sea and Land Air.—Having learned that on the Baltic, asthmatic invalids were much better at sea than on shore, M. Vogel analysed the air a league from the shore, and concluded, 1. That the air above the Baltic, a league from the shore, contains less carbonic acid than the ordinary atmosphere, and carbonic acid probably diminishing as we recede from the land; and, 2. That the same air contains muriates in greater or lesser quantities.—*Ibid.*

State of the Skin, &c. in Yellow Fever.—From the facts observed by himself, or recorded by others, Dr. Desmoulins has been led to the following conclusions with regard to the biliary secretion and yellowness of the skin:—

1. That, in yellow fever, there is no increase of the secretion of bile.

2. That the black vomit and dark-coloured stools are the produce of exhalation from the surface of the intestines.

3. That the yellow colour of the skin is produced by the blood in the minute vessels of the cutis, which are in a state of congestion, analogous to that which, in the mucous membranes, gives rise to hemorrhage.

4. That the firmer texture of the skin alone prevents this likewise from ending in a similar discharge.

5. That the yellow colour, which is almost always preceded by petechiæ, is only a sort of general ecchymosis.

6. That the yellow fever is nothing else than a congestion or determination of blood to the skin and mucous membranes simultaneously, principally to those of digestion.—*Lond. Med. and Phys. Journal, Oct.* 1822.

Treatment of the Barcelona Fever.—Dr. Francais has read to the Academy of Medicine a memoir on this subject. Blood-letting, which has been frequently employed and regarded as highly useful in America, has always proved deleterious in Spain. The tonic and revulsive treatment has succeeded best, particularly the exhibition of cinchona, and the application of the moxa along the spinal column. It may readily be supposed that the means of employing these universally did not exist, but, according to Dr. Francais, the trials made were sufficient to demonstrate the happy effects resulting from this plan..—*Ibid.*

Effects of drinking Boiling Water.—It is the custom of some poor and thoughtless persons to suffer children to drink from the spout of a tea-kettle while filling it at the pump; they have afterwards attempted to drink when it has just been taken from the fire, supposing it still to contain cold water. No less than four cases of this kind are related in the *Medico-Chirurgical Transactions*, by Dr. Hall. The symptoms produced are those of *croup*, that is, of inflammation of the glottis and larynx, and not, as might have been expected, of the œsophagus and stomach. It appears, indeed, probable, that the boiling water does not actually reach the stomach, but that it is arrested by spasm of the pharynx. Dr. Hall recommends an incision into the windpipe, but the only case of this operation which he relates, proved, as might have been expected, fatal. Where the injury is extensive, there seems to be no remedy.—*Brande's Journal of Science, No.* 27.

Cure of Ring-worm.—Mr. T. J. Graham, of Cheltenham, recommends the lime-water which has been used for purifying gas, as a very efficacious remedy in the above troublesome disease. The head is to be well cleansed morning and evening with soap and water, and afterwards washed with the lime-water from the gas-works. (*Monthly Mag. Sept.*) The above *lime-water* is a very heterogeneous compound, so that it is impossible to say which of its ingredients is effectual. It contains lime, ammonia, sulphuretted hydrogen, volatile oil, and, probably, several other compounds of a more complex nature.—*Ibid.*

Mineralogy.—A work on the science of mineralogy is about to make its appearance in Germany. It is from the pen of Mr. Frederick Mohs, Professor of Mineralogy at Fryberg, and will contain the terminology, the rules of the construction of Mr. Mohs' system, and the nomenclature, the characteristic and the descriptive part of natural history. The whole to be comprised in two volumes octavo, with plates.—An English translation will appear at the same time, made under the inspection of the author, by Mr. Haidinger, who lately visited this country, in company with Count Brenner. From the known celebrity of the Fryberg school as well as the acknowledged ability of the author, we have reason to expect that this will be a useful work.—*Ibid.*

Experiments on Incubation.—M. Geoffroy Saint-Hilaire has communicated to the Institute of France an interesting account of different methods by which *oviparous* animals may be rendered *viviparous.* These experiments completely succeeded with regard to water-snakes (*couleuvres acquatiques,*) when placed in a dry situation, and under circumstances unfavourable for dropping their eggs. These were retained by the parent, and preserved in the genital receptacle; so that, at the end of a certain period of this uterine incubation, the young came forth from the eggs and the body of their mother. These experiments were not followed with equally successful results in those animals whose eggs have calcareous shells. Notwithstanding his endeavours, M. Geoffry has not succeeded in effecting the production of young chickens alive; but he is led, from his observations, to believe that this might be done by better contrived means.

Rudiments of a Fœtus in the Testicle.—Dr. Friedlander relates that a child was born in December, 1817, at a village near Glogau, which at first appeared in all respects healthy and natural. In the month of May following, it was seized with difficulty in making water, and the right testicle began to enlarge. By the 19th of June, the testicle had so much increased as to descend to the knee. On the 19th of July, a ligature was applied near the ring, which came away on the 22d, and the child did well. The testicle was four inches three lines (Rhine measure) in length, two inches four lines in breadth; its weight, seven ounces; the epidydimis was entirely wanting. After careful examination of the tunica vaginalis, a hard body was perceived, which proved to be an os femoris, eighteen lines long, and without periosteum. Several other bones were subsequently found, connected, by means of cellular texture and muscular fibres, in such a manner as to form the pelvis and right lower extremity of a fœtus about four months old. The left branch of the os pubis and os ischium were wanting; the ileo-femoral articulation presented a triangular appearance; the sacrum was distinct, with its surface for receiving the first bone of the spinal column; in the middle of the pelvis was a ligamentous mass, about an inch long, resembling the rudiments of the lumbar vertebræ. The tibia and fibula were naturally formed; the interosseous ligament "a little too thick."

METEOROLOGICAL OBSERVATIONS

FOR SEPTEMBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.						Thermometer			Winds.			Rain Guage.	Hygrometer.	Weather.
	7 A M		2 P M		9 P M		7 AM	2 PM	9 PM	7 A M	2 P M	9 P M			
1	30	37	30	30	30	22	70	85	74	se	se	se	—.05	685	variable
2	30	10	30	04	30	00	76	81	70	s	s	nw	—.90	724	cloudy
3	29	90	30	00	30	00	69	87	74	sw	sw	sw		704	variable
4	30	08	30	20	30	20	66	82	67	sw	sw	sw		684	clear
5	30	26	30	26	30	26	62	81	69	sw	sw	sw		656	do
6	30	26	30	26	30	26	65	82	71	ne	sw	sw		645	do
7	30	26	30	26	30	15	68	88	76	sw	sw	sw		636	do
8	30	15	30	15	30	15	70	88	75	nw	sw	s		638	cloudy
9	30	10	30	10	30	22	72	85	76	sw	s	s		672	do
10	30	20	30	20	30	20	70	90	76	s	s	sw	—.01	690	clear
11	30	14	30	14	30	14	75	94	78	sw	sw	sw		687	cloudy
12	30	27	30	27	30	27	76	91	77	sw	sw	sw		660	clear
13	30	27	30	27	30	27	78	94	79	sw	sw	nw		684	clear
14	30	27	30	20	30	20	72	84	72	sw	se	sw	—.31	695	cloudy
15	30	14	30	14	30	00	74	90	74	sw	sw	nw		670	clear
16	29	90	29	90	30	10	72	82	69	sw	nw	nw		638	do
17	30	30	30	44	30	56	60	68	60	nw	nw	sw		622	do
18	30	64	30	60	30	52	54	71	62	nw	sw	sw		619	do
19	30	38	30	38	30	30	64	79	68	sw	sw	nw	—.38	660	cloudy
20	30	00	30	00	30	00	69	75	69	sE	nw	nw	1.62	680	do
21	29	90	30	00	30	12	65	72	62	sw	sw	nw		650	clear
22	30	36	30	36	30	40	58	70	62	nw	nw	nw		645	do
23	30	57	30	57	30	57	55	70	60	nw	nw	nw	—	642	do
24	30	50	30	50	30	50	58	73	62	nne	ne	sw		648	do
25	30	44	30	44	30	44	59	74	68	cal.	sw	sw		650	do
26	30	44	30	30	30	30	64	75	73	sw	sw	sw	—.07	718	cloudy
27	30	30	30	16	30	16	70	77	74	sw	s	s		800	do
28	30	16	30	16	30	00	73	83	75	s	se	se	—.03	832	do
29	30	00	30	00	30	00	73	78	72	s	se	se		834	do
30	30	00	30	00	30	12	68	77	70	nw	nw	nw		750	clear

The quantity of rain in this month, amounted to 3 inches and 40—100ths.

The thermometrical range for September was considerably higher than for the same month in the preceding year; it was unusually moist, especially towards the latter part. Very thick fogs and light winds, on the mornings of the 3d, 9th, 24th, and 26th. The autumnal equinox made its appearance at 10 P. M on the 19th, and continued until the evening of the 20th. The sun was obscured the greatest part of the 14th, and completely so on the 20th, 26th, and 27th. The thermometer, placed in the sun, on the 6th, at 2 P. M. stood at 106°. Thunder, lightning, and rain, on the 2d and 14th. Vespertive lightning on the 11th and 13th. The rain on the 1st, 10th, and 19th, fell in the night.

METEOROLOGICAL OBSERVATIONS

FOR OCTOBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Guage.	Hygrometer.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 AM	2 PM	9 PM			
1	30 27	30 27	30 27	56	65	56	nw	nw	nw		710	clear
2	30 30	30 30	30 46	53	58	54	nw	ne	ne		696	cloudy
3	30 47	30 40	30 36	51	62	55	ne	ne	ne		690	variable
4	30 30	30 30	30 30	54	72	62	ne	ne	ne		687	clear
5	30 30	30 30	30 30	58	73	63	ne	ne	ne		693	do
6	30 30	30 30	30 30	58	73	61	ne	se	se		710	do
7	30 25	30 19	30 10	65	78	68	se	se	se		724	cloudy
8	30 08	30 08	30 08	65	80	65	nw	nw	nw	—.07	730	clear
9	30 20	30 20	30 18	65	73	63	nw	sw	sw		730	do
10	30 15	30 15	30 10	67	75	70	sw	sw	sw	—.11	735	cloudy
11	30 04	30 00	30 00	70	78	72	sw	sw	sw	—.03	780	do
12	30 00	30 00	30 15	65	74	61	sw	sw	sw		825	do
13	30 25	30 2	30 30	64	74	58	sw	sw	sw		770	clear
14	30 35	30 4	30 45	51	66	56	ne	ne	ne		732	cloudy
15	30 45	30 45	30 45	49	63	54	nw	nw	nw		686	clear
16	30 45	30 40	30 33	52	70	62	se	w	w		672	cloudy
17	30 22	30 20	30 20	56	73	62	w	w	sw		682	clear
18	30 20	30 14	30 10	59	78	67	sw	sw	sw		687	cloudy
19	30 10	30 00	30 00	63	83	70	sw	sw	sw		725	do
20	30 00	29 97	29 86	71	84	72	sw	sw	sw	—.02	732	do
21	29 75	30 00	30 20	71	68	62	s	nw	nw		686	do
22	30 26	30 34	30 40	52	75	49	nw	nw	nw		680	clear
23	30 40	30 40	30 40	37	58	45	n	n	n		657	do
24	30 40	30 34	30 22	43	54	44	n	n	n		632	cloudy
25	30 20	30 30	30 36	40	54	42	ne	ne	ne		623	do
26	30 40	30 40	30 40	36	54	46	ne	ne	ne		622	clear
27	30 35	30 18	30 04	42	49	46	ne	ne	ne	1. 42	640	cloudy
28	29 68	29 60	29 60	49	55	46	ne	nw	nw	—.67	655	do
29	29 60	29 80	29 87	45	54	45	w	w	w		650	do
30	29 95	29 90	30 03	41	54	48	w	w	w		650	clear
31	30 05	30 00	30 00	42	68	56	sw	sw	sw		650	do

The quantity of rain which fell this month, is 2 inches and 32—100ths.

The weather of the month of October, was, for the most part, pleasant and dry. The nights were generally clear and starry. On the mornings of the 4th and 18th, there was heavy dews, with light winds. The sun was obscured on the 2d, 11th, 14th, 27th, and 28th. On the 22d there was ice at the villages of Bloomingdale and Hoboken, a few miles from this city, but the thermometer, it will be seen, stood at 37° on the same morning, in a S. E. exposure, at 7 o'clock. The rain on the 8th, fell a short time before sunrise.

METEOROLOGICAL OBSERVATIONS

FOR NOVEMBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.						Thermometer.			Winds.			Rain Guage.	Hygrometer.	Weather.
	7		2		9		7	2	9	7	2	9			
	A	M	P	M	P	M	AM	PM	PM	AM	P M	PM			
1	29	90	29	94	30	05	44	51	44	nw	nw	w		644	cloudy
2	30	05	30	05	30	00	38	54	46	w	w	w		638	variable
3	30	00	30	18	30	34	44	61	45	w	w	n		635	clear
4	30	44	30	44	30	34	36	60	43	n	n	n		630	do
5	30	44	30	44	30	38	35	58	48	n	n	n		632	do
6	30	30	30	28	30	28	44	59	51	nw	nw	nw		645	cloudy
7	30	28	30	2[illegible]	30	16	49	59	56	nw	sw	sw	—.66	682	do
8	30	04	30	18	30	34	56	75	60	sw	sw	sw	—.13	695	do
9	30	48	30	50	30	56	44	51	45	ne	ne	ne		687	do
10	30	67	30	67	30	64	40	50	49	ne	ne	ne		690	do
11	30	50	30	46	30	32	48	61	56	ne	ne	ne		710	do
12	30	30	30	30	30	30	51	60	58	se	se	se		758	do
13	30	30	30	18	30	08	56	49	48	ne	ne	ne	2.80	758	do
14	30	00	29	82	29	98	48	51	42	ne	nw	nw	—.57	763	do
15	30	24	30	36	30	42	37	58	46	nw	nw	sw		746	clear
16	30	48	30	48	30	48	42	61	50	sw	sw	sw		734	do
17	30	48	30	48	30	40	41	64	42	sw	sw	sw		728	do
18	30	36	30	36	30	29	45	51	47	ne	ne	ne	—.78	742	cloudy
19	30	29	30	29	30	34	45	54	47	nw	nw	nw		741	variable
20	30	34	30	30	30	26	44	58	50	nw	nw	nw		741	cloudy
21	30	20	30	20	30	14	50	61	55	sw	sw	sw		741	do
22	30	00	29	84	29	70	46	47	44	n	ne	ne	1.94	746	do
23	29	60	29	98	30	08	44	57	44	w	w	w		735	do
24	30	20	30	38	30	56	40	46	40	nw	nw	nw		716	variable
25	30	56	30	30	30	26	35	59	44	nw	sw	sw		727	do
26	30	26	30	26	30	26	36	44	39	nw	nw	nw	—.20	710	do
27	30	26	30	26	30	26	34	45	40	nw	w	w		705	clear
28	30	16	29	90	29	98	40	53	50	s	s	s	—.36	736	cloudy
29	30	00	30	00	29	95	47	58	45	w	w	w	—.10	765	do
30	29	90	29	84	29	84	56	69	61	s	s	s	—.31	774	do

The quantity of rain which fell this month, was unusually great, being 7 inches and 85—100ths.

The 8th was very sultry, with vespertine lightning, and rain, at 9 P. M The grass and house-tops were white with hoar frost on the mornings of the 4th, 5th, and 17th. On the 18th, moderate rain all day, with lightning in the S. W at 7 P. M. The sun was partially obscured on the 29th and 30th, and totally on the 7th, 10th, 11th, 13th, 14th, 18th, 21st, and 22d. On the 26th the sun was overclouded a short time after it had risen; rain soon commenced, intermingled with large flakes of snow, which instantly melted when they came in contact with the earth: it continued until 11 A. M. when a clear afternoon and a starry night succeeded.

OBITUARY.—Died, on Thursday the 5th of December 1822, of Phthisis Pulmonalis, JACOB DYCKMAN, M. D. Commissioner of Health, Surgeon to the New-York Alms House, Secretary of the Literary and Philosophical Society of New-York, Member of the New-York Historical Society and of the Lyceum of Natural History, and one of the Editors of the New-York Medical and Physical Journal.

In recording the death of their beloved and lamented Colleague the surviving Editors cannot, in justice to their feelings, withhold the tribute of their testimony to his worth.

Jacob Dyckman was born in the County of West Chester, in 1788, and being early destined for a liberal profession, was carefully prepared by the usual preliminary studies for Columbia College, which he entered in the autumn of 1806. While in this distinguished seminary he endeared himself to all by the suavity of his manners and strict propriety of conduct, and particularly gained the good will of his preceptors, by vigorous and well sustained diligence. He graduated in 1810, and after a few months attention to his health, which at this time first became delicate, he commenced his professional studies under the direction of Dr. Hosack. Here he continued to support the high moral character which he had before acquired, with uniform evidences of respect and esteem from his associates.

He graduated as Doctor of Medicine in 1813, in the College of Physicians and Surgeons in the University of the State of New-York; on which occasion he prepared for publication an Inaugural Dissertation on the Pathology of the Human Fluids.

On the merits of this production the public opinion has been distinctly pronounced. It contains, without doubt, the best exposition of some of the doctrines taught in the University of New-York, by the distinguished Professor of the Practice, and has contributed in an eminent degree, to the high popularity which they now enjoy. It manifests, throughout, an extensive and intimate acquaintance with medical literature, and a capacity for analytical investigation, worthy of riper years. In compliance with the advice of his friends and the public expectation, it was the intention of the author to have extended his inquiries, and augmented his work, in a new and improved edition.

A review of this Dissertation contained in the Philadelphia Journal of the Medical and Physical Sciences, thus concludes, after

combating the principles it supports: "We have rarely read an early production of the kind, which displays more recondite research, more dexterity of statement, more ingenuity of argument, or plausibility of style and manner. It is an able defence of the cause it espouses, and affords an earnest of future excellence, which no one will more delight to welcome and celebrate than ourselves."—Vol. iv. p. 370.

His constitution again became enfeebled by a pulmonary attack, which, however, was not so severe as to prevent him from entering on the practice of his profession; nor did it interfere with his devotion to pursuits subservient to the cause of humanity. These he continued diligently to prosecute, and in the year 1818, he published an edition of Dr. Duncan's Dispensatory, to which he added many valuable notes, which manifested much botanical research, and a familiar acquaintance with the indigenous medical resources of our country. His attachment to these pursuits was unremitted, and he had been engaged for some time in collecting materials for a Medical Botany of the United States.

He was successively appointed Physician to the New-York Dispensary, Surgeon to the New-York Alms House, and Commissioner of Health; in all which situations the public confidence in his fidelity and zeal, was never for a moment impaired.

At the commencement of the present year, the New-York Medical and Physical Journal was planned, and much was anticipated by the present editors, from the diligence and qualifications of their mutual friend; but there are strong reasons for believing from the great feebleness which almost immediately ensued, that even then the irresistible mandate had been issued which was to call him from all earthly communion.

All who were acquainted with the deceased will delight to dwell on the amenity of his disposition, and the blameless tenor of his life. Remarkably free from the malignant passions, his heart was the seat of generous feelings, and was ever alive to the sensibilities of humanity. In every sphere in which he moved, his worth was confessed; and in every situation to which private confidence or public favour called him, his zeal and industry were incessant and unwearied. He has left behind him many connected by the endearments of friendship: none who can deny the benevolence of his heart, or the purity of his character.

Notice to Correspondents.

In order to insert the interesting communications of Drs. Bayley and Walters, we have been compelled to omit several papers on Natural History and a review of Professor Warren's Treatise on the Nervous System, although we have exceeded our customary limits.—In our next number we shall also notice the late republications of Cooper's Surgery, and the Surgical Dictionary ; the former with notes by Dr. Stevens, and the latter with additions by Professor Anderson.

We have observed that several editors of periodical works are in the habit of selecting original articles from this Journal, and that without any acknowledgment. This conduct is uncandid and illiberal ; and whether the guilt rests with the editors alluded to, or results from the writer's desire to see his essay more extensively circulated, the consequences are the same to us. Hereafter communications forwarded to us must be sent to no other journal; or if copied, it is expected that the obligation will be stated.

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J. Seymour, Printer, 49 John-street.